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AND

JOURNAL OF SCIENCE.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

VOL. VIII.—FOURTH SERIES.

JULY—DECEMBER, 1854.

"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

———

—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

CONTENTS OF VOL. VIII.

(FOURTH SERIES.)

NUMBER XLIX.—JULY 1854.

NUMBER L.—AUGUST.

NUMBER LI.—SEPTEMBER.

NUMBER LII.—OCTOBER.

NUMBER LIII.—NOVEMBER.

NUMBER LIV.—DECEMBER.

NUMBER LV.—SUPPLEMENT TO VOL. VIII.

PLATES.

ERRATA.

Page 308, line 17 from top, *for P. bina,* an unnamed species &c. *read P. ——— ?* an unnamed species &c.

Page 336, line 7 from top, *for* September *read* November.

ALERE FLAMMAM.

I. *On the Vibrations and Tones produced by the Contact of Bodies having different Temperatures.* By JOHN TYNDALL, *Ph.D., F.R.S., Member of the Royal Society of Haarlem, and Professor of Natural Philosophy in the Royal Institution*[*].

IN the year 1805, M. Schwartz, inspector of one of the smelting-works of Saxony, having a quantity of silver in a ladle which had just solidified after melting, and, wishing to hasten its cooling, placed it upon a cold anvil, when to his astonishment sounds, which he compared to those of an organ, proceeded from the mass. The rumour of this discovery excited the curiosity of Professor Gilbert, the editor of Gilbert's *Annalen,* and in the autumn of the same year he paid a visit to the smelting-works in question. He there learned that the piece of silver from which the sounds proceeded was cup-shaped, had a diameter of 3 or 4 inches and a depth of half an inch. Gilbert himself, under the direction of M. Schwartz, repeated the experiment. He heard a distinct tone, although nothing that he could compare to the tone of an organ. He also found that the sound was accompanied by the quivering of the mass of metal, and that when the vibrations of the mass ceased, the sound ceased likewise. The Professor limited himself to the description of the phænomenon, and made no attempt to explain it.

In the year 1829 Mr. Arthur Trevelyan was engaged in spreading pitch with a hot plastering iron, and observing in one instance that the iron was too hot, he laid it slantingly against a block of lead which happened to be at hand. Shortly afterwards he heard a shrill note, resembling that produced on the chanter

of the smaller Northumberland pipes, an instrument played by his father's gamekeeper. Not knowing the cause of the sound, he thought that this person might be practising out of doors; but on going out the tone ceased to be heard, while on his return he heard it as shrill as before. His attention was at length attracted to the hot iron, which he found to be in a state of vibration, and thus discovered the origin of this strange music. In 1830 he came to Edinburgh, and mentioned the fact to Dr. Reid; the latter, not knowing what Schwartz and Gilbert had observed previously, regarded the phænomenon as new, and recommended Mr. Trevelyan to investigate it more fully; Mr. Trevelyan did so; among other things he discovered the form to be given to the vibrating mass (the rocker) in order to obtain the effect with ease and certainty. The results of his numerous and well-contrived experiments were communicated to the Royal Society of Edinburgh, and were subsequently printed in the Society's Transactions.

On the 1st of April 1831 these vibrations and tones constituted the subject of a Friday evening's lecture by Professor Faraday at the Royal Institution. The following extract from the Journal of the Institution (vol. ii. p. 120) informs us of the views of the philosopher last mentioned with respect to the cause of the tones:—"As the sounds were evidently due to the rapid blows of the rocker, the only difficulty was to discover the true cause of the sustaining power by which the rocker was kept in motion, whilst any considerable difference of temperature existed between it and the block of lead underneath. This power Professor Faraday referred to expansion and contraction, as Professor Leslie and Mr. Trevelyan had done generally. But he gave a minute account of the manner in which, according to his views, such expansions and contractions could produce the effect. . . . . The superiority of lead, as a cold metal, he referred to its great expansibility by heat, combined with its deficient conducting power, which is not a fifth of that of copper, silver or gold; so that the heat accumulates much more at the point of contact in it than it could do in the latter metals, and produces an expansion proportionably greater."

Professor J. D. Forbes was present at this lecture, and by it, apparently, he was induced to undertake the further examination of the subject. On the 18th of March and on the 1st of April 1833 the results of his inquiries were communicated to the Royal Society of Edinburgh. He dissents from the explanation supported by Professor Faraday. The vibrations, he urges, are dependent for their existence on the difference of temperature of the two surfaces in contact; if then the heat accumulate at the surface of the cold metal, its effect will be to bring both surfaces to

a common temperature, and thus to stop the vibrations, instead of exalting them, as supposed by Professor Faraday. Again, if the phænomenon be due to expansion, the greater the expansion the greater ought to be the effect; but the expansion depends upon the quantity of heat transmitted from the hot rocker to the cold block during their contact, and this again upon the conductivity of the block; so that instead of being a bad conductor, the block, to produce the greatest effect, ought to be the best conductor possible. The idea of an accumulation of heat at the surface being more favourable to the action than a rapid communication with the interior, Professor Forbes regards as an "obvious oversight*."

Having thus, to all appearance, overturned the views previously entertained, Professor Forbes proceeds to found a theory of his own. His experiments have led him to the enunciation of certain "general laws," and these converge upon the still more general conclusion,—" that there is a repulsive action exercised in the transmission of heat from one body into another which has a less power of conducting it." This repulsion Professor Forbes considers to be "a new species of mechanical agency in heat;" and he cites the remarkable experiments of Fresnel, on the mutual repulsion of heated bodies *in vacuo*, as bearing directly upon the subject.

Such, apparently, was the unsettled state of the question when my attention was drawn towards it last summer. The possibility of the explanation offered by Professor Forbes, affording, as it seemed to do, a chance of becoming more nearly acquainted with the intimate nature of heat itself, was a strong stimulus to inquiry. I was not aware, until informed of it by my friend Professor Magnus, that Seebeck had further examined the question, and substantiated the conclusions arrived at by Faraday. On reading Seebeck's interesting paper, I found that he had already obtained many of the results which it was my intention to seek; nevertheless the portion of the subject which still remained untouched presented sufficient interest to induce me to prosecute my original idea.

I purpose in the present memoir to examine the experimental basis of those laws which Professor Forbes regards as establishing the existence of "a new mechanical agency in heat;" and as I am anxious to place it within the power of every experimenter to test the results to be communicated, I shall connect with each series of experiments a sufficiently exact description of the instruments made use of.

The first general law enunciated by Professor Forbes is as follows:—

* Philosophical Magazine, Series 3, vol. iv. pp. 15 and 182.

"*The vibrations never take place between substances of the same nature.*"

Let us see whether this law will bear the test of experiment.

I. *Iron Rocker.*

Fig. 1 represents a sketch of the rocker; the length AC is 5·1 inches; the width AB 1·85, and the length of the stem EF is 12 inches. Fig. 2 is a transverse section of the rocker, showing
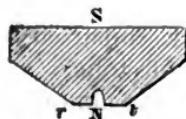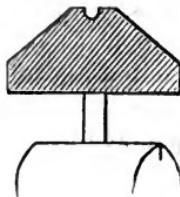
Fig. 2.   Fig. 4.   Fig. 1.
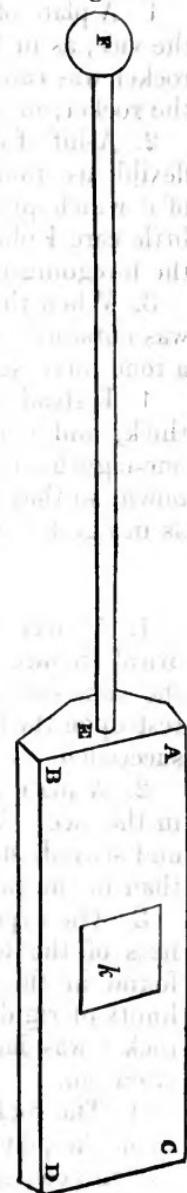
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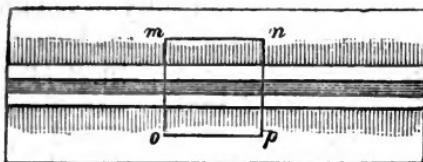
the groove underneath; the depth SN is 0·5 of an inch. The distance *rt* is 0·35 of an inch, and it is divided into three equal spaces, the central one of which is occupied by the groove, and the other two are the surfaces which rest upon the bearer. Fig. 3 is a plan of the rocker turned upside down. The spaces *k* (fig. 1) and *mnop* (fig. 3) will, for the present, be left out of consideration.

1. The blade of a dinner-knife was fixed in a vice so that the edge was horizontal. The rocker was laid upon the edge, and the stem suitably supported. On stirring the rocker, a loud and musical sound commenced, and continued for a considerable time.

2. The knife was removed, and a plate of sheet iron $\frac{1}{40}$th of an inch in thickness was fixed in the vice; the hot rocker was caused to rest upon the edge of the plate. On stirring the rocker, vibrations, accompanied by a musical tone, were set up as before.

3. The experiment was repeated with a second plate of iron $\frac{1}{100}$th of an inch in thickness, and a still better tone was obtained :—when the rocker rested on a *block* of iron, the vibrations were not permanent.

II. *Copper Rocker.*

The pieces *k* and *mnop* (figs. 1 and 3) are plates of copper screwed tightly on to the surface of the iron. In this way a single rocker is made to do the duty of two.

1. A plate of copper $\frac{1}{60}$th of an inch in thickness was fixed in the vice, as in the former cases; and the copper portion of the rocker was caused to rest upon it. A slight shock, imparted to the rocker, immediately excited a strong and durable tone.

2. A bit of copper foil was fixed in the vice; it was almost as flexible as stout foolscap paper, but to give it rigidity the height of it which projected above the vice was very minute. With a little care I obtained tones stronger and more musical than in the foregoing instance.

3. When the rocker was laid upon a *block* of copper, no tone was obtained; and it was found that the difficulty of obtaining a tone increased as the plate made use of became thicker.

4. Instead of the plate, two wires of copper $\frac{1}{23}$th of an inch thick, and pointed with a file, were fixed in the vice at about one-eighth of an inch apart. The rocker was turned upside down, so that the flat surface of the copper *k* rested on the wires as in fig. 4 :—forcible vibrations were obtained in this way.

III. *Brass Rocker.*

1. A piece of brass tube was fixed in the vice, and its cylindrical surface rendered clean by a fine file. A brass rocker, of the same dimensions as that represented in fig. 1, was caused to rest upon the tube; on stirring the rocker, continuous vibrations succeeded.

2. A plate of brass $\frac{1}{100}$th of an inch in thickness was fixed in the vice. The rocker being laid upon the edge of the plate and stirred, stronger and more durable vibrations were obtained than in the case of the tube.

3. The experiment was repeated with a plate twice the thickness of the former; distinct vibrations were obtained. It was found in this case also that the thinner the plate, within its limits of rigidity, the more decided were the effects :—when the rocker was laid upon a *block* of brass, there was no permanent vibration.

4. The rocker was turned upside down, its flat surface resting upon the points of two common brass pins; a constant rocking was the consequence.

IV. *Silver Rocker.*

This instrument was formed by attaching a piece of silver to the brass rocker used in the last experiments, exactly as the piece of copper, *mnop*, was attached to the iron rocker, fig. 1. The silver partook of the general shape of the under surface of the rocker, being bevelled off on both sides of the groove passing through its centre.

1. A strip of silver about $\frac{1}{100}$th of an inch in thickness was fixed in the vice, and the silver portion of the rocker was caused to rest upon the edge of the strip. On shaking the rocker, a fine mellow musical tone was obtained.

2. A new half-crown was fixed in the vice, and the rocker caused to rest upon the milled edge; no permanent vibrations were obtained. A similar difficulty was encountered with the edge of a shilling. On the edge of a sixpence, a feeble though distinct vibration was obtained.

3. When the edges of the coins were beaten out with a hammer, and thus rendered thin, distinct vibrations were obtained with all of them. I do not assert the impossibility of obtaining vibrations on the edge of a half-crown, but merely state that with the same rocker vibrations were obtained upon a thin edge of silver, and not upon a thick one.

4. The rocker was placed against a block of silver weighing about 10 oz.; no permanent vibrations were obtained.

V. *Zinc Rocker.*

The instrument is of the same size and shape as the iron rocker, fig. 1, except that the depth, SN fig. 2, is less, and the mass therefore lighter*.

Placed upon the edge of the thinnest sheet zinc, the edge having been sharpened by a file, distinct musical tones were obtained:—on a *block* of zinc the rocker refused to vibrate permanently.

Tin Rocker.

A cake of tin, formed by pouring the molten metal upon a smooth flat surface, was heated and balanced upon two small protuberances of a second piece of the same metal. Continuous rockings were immediately set up. I met the mass by accident in the laboratory, and having obtained the vibrations without changing its shape, I did not think it necessary to strengthen the action by forming it into a regular rocker.

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* Better results are obtained when the rocker is still further lightened, by scooping away part of its central mass, thus making its upper surface concave instead of flat.

The number of metals capable of this action might, I doubt not, be greatly extended.   Thus far we have obtained vibrations with

> Iron upon iron,
> Copper on copper,
> Brass on brass,
> Silver on silver,
> Zinc on zinc,
> Tin on tin ;

and these, I think, are sufficient to show that *the first general law of* Professor Forbes *does not stand the test of experiment.*

Seebeck indeed had already proved the untenableness of this law.   His method of experimenting has been followed in one or two of the cases above described.   The placing of the heated rocker upon pointed wires is his idea.   Rockings are very readily obtained in this way; but when *tones* are required, the sharp edge will, I think, in general be found preferable.

The second general law of action stated by Professor Forbes is as follows :—

" Both substances must be metallic."

This is the case which first excited my attention; for even granting the final explanation given by Professor Forbes to be the true one, the necessity of the law before us does not at all follow.   Previous to entering upon the present subject, I had found that rock-crystal and rock-salt possessed conducting powers not much, if at all, inferior to some of the metals; and this led me to suppose that either or both of these substances might possibly be made to exhibit the action which the above law restricts to metals.

My first attempts failed through want of delicacy, as first attempts generally do.   But a little practice suggested the means of imparting to the rocker the requisite degree of mobility. Crystals of quartz were cut in such a manner, that when the rocker was laid upon them, a very slight force was sufficient to cause it to oscillate.   By this means I had the satisfaction of obtaining distinct vibrations from a brass rocker placed upon rock-crystal.

I refrain from entering into a more exact statement of the manner in which the crystals were cut; for subsequent experience proved that there is no difficulty in obtaining the effect, without any artificial preparation whatever.

I shall now proceed to describe the results obtained with non-metallic bodies.

1. *Rock-crystal.*—The brass rocker already described was heated, and placed upon the natural edge of the prism ; the

stem was supported by a knife-edge, so that the rocker lay nearly horizontal : a strong tone was thus obtained.

Vibrations also followed when the rocker was laid upon the edge of the pyramid which caps the hexagonal prism.

The experiments were repeated with fumy quartz, and the same result was obtained.

2. *Fluor-spar.*—A smaller brass rocker than that last used was found to answer best with this crystal. The dimensions corresponding to AC, AB, fig. 1, were 3·8 in. and 1·25 in. respectively; while the depth was the same, or nearly so. This rocker having been placed upon the natural edge of the crystalline cube, a clear and melodious note was instantly produced. Forcible vibrations were also obtained with the larger rocker, but not so clear a tone.

The angle of the cube was cloven off so as to expose the edge of the octahedron; on this edge also vibrations were obtained.

Fortification Agate.—Distinct vibrations and tones were obtained with the large brass rocker. I found it sometimes convenient to increase the time of oscillation by laying a thin brass bar, with small knobs at the ends, across the rocker—a mode of experiment due, I believe, to Mr. Trevelyan. With this precaution, in the case before us, the rocker continued swinging for nearly half an hour; and when it ceased, it was under the temperature of boiling water.

Rock-salt.—The rocker used in the foregoing experiment was laid aside, and a piece of rock-salt was prepared for trial. The mass was cloven so as to exhibit the surfaces of the primitive cube, and was so placed that the straight line formed by the intersection of two of the surfaces of the cube was horizontal. Previous to heating the rocker, I laid it, according to practice, upon the mass, merely to ascertain whether the arrangement was likely to answer. To my astonishment a deep musical sound commenced immediately. The temperature of the rocker was at this time far below that of boiling water, and when it had ended its song it was scarcely above a blood-heat.

The heated rocker was laid upon a large boulder-shaped mass of the salt; it commenced to sing immediately. I scarcely know a substance, metallic or non-metallic, with which vibrations can be obtained with greater ease and certainty than with this mineral. To the remarkable properties which the researches of Melloni have shown to belong to rock-salt, a new one may now be added.

Aventurine.—I was tempted to try this mineral from having met a piece of it possessing a clean sharp edge. The large brass rocker, placed hot upon this edge, gave a decided tone.

Sulphate of Potash.—Care is required with this artificial cry-

stal, as it readily flies to pieces on the sudden communication of heat. With proper precautions, feeble, but well-established vibrations were obtained.

Onyx.—A distinct tone.

Tourmaline.—After many trials I obtained a continuous vibration and low tone.

Fossil Wood.—Two different specimens were examined, and distinct tones obtained with both of them.

Banded Agate.—Strong and continuous vibrations, when the rocker was very hot.

Chalcedony.—Loud and long-continued knockings on a knob of this mineral.

Glass.—Decided vibrations on the smooth rounded edge of the foot of a drinking-glass. Mr. Trevelyan believed that he once obtained vibrations upon glass, but the fact is doubted by Professor Forbes. This is the only experiment on non-metallic bodies, as far as I am aware of, hitherto on record.

Earthenware.—A feeble tone, which soon ceased, was obtained on the edge of a dinner plate.

Flint.—A decided tone, though not so strong as that obtained from rock-crystal.

Lydian Stone.—Permanent vibrations.

Heliotrope.—A durable tone.

Iceland-spar.—A lighter rocker than any of those hitherto described was found necessary in experimenting with this crystal. The mass is soft, and is readily bruised by the rocker when the latter is heavy. With a suitable instrument a continuous feeble tone was obtained.

Red Hæmatite.—Distinct tones were produced by several specimens of this mineral.

Arseniacal Cobalt.—A strong tone.

Meteoric Iron from Mexico.—A low musical tone.

This list might be readily extended. The substances mentioned in it were chosen on account of their accidentally presenting the conditions favourable to experiment. The principal condition is a clean even edge. Several of the minerals possessed such edges cut artificially; others possessed them naturally. In the case of chalcedony, the rocker was placed upon a rounded knob; in the case of tourmaline, one of the ridges, which usually run along the surface of the prism, served as a support; with glass and earthenware, the surfaces were smooth and rounded. As a general rule, however, I have found an even edge best. With such an edge, and rockers similar to those described, no difficulty will be experienced in repeating and extending these experiments.

It is usual to permit the knob at the end of the handle of the

rocker to rest upon a flat surface, while the instrument itself leans slantingly against the bearer. In delicate experiments, I think a knife-edge is a better support for the handle, the rocker being placed horizontal, or nearly so.

Omitting the last three substances, which might, perhaps, with some justice be regarded as metallic, we find a number of exceptions to the law under consideration, which far exceeds the number of bodies mentioned in the paper of Professor Forbes. *These exceptions demonstrate that the second law also is untenable.*

The third general law runs as follows :—

" *The vibrations take place with an intensity proportional (within certain limits) to the difference of the conducting powers of the metals for heat, the metal having least conducting power being necessarily the coldest.*"

The evidence adduced against the validity of the first law appears to destroy this one also; for if the vibrations are to be ascribed to a difference in the conducting powers of the rocker and bearer, then when there is no such difference there ought to be no vibrations. But we have shown, in half a dozen cases, that vibrations occur when rocker and bearer are of the same metal. The same facts deprive the latter part of the third law of its significance.

I will however cite one or two experiments, in which the conditions regarded necessary by Professor Forbes were reversed, and the effect was produced notwithstanding.

1. Silver stands at the head of the conductors of heat. A copper rocker was laid upon the edge of a thin plate of this metal; strong musical notes were obtained from the arrangement.

2. Forcible vibrations were produced by placing a brass rocker upon the same silver plate.

3. A feeble, but distinct tone, was produced by the iron rocker.

4. Gold is a better conductor than brass; nevertheless strong vibrations were obtained by placing a hot brass rocker upon the edge of a half-sovereign.

These experiments are, I think, sufficient to prove the non-existence of the third law.

In the prosecution of his inquiry, Professor Forbes discovered " that at least two metals were perfectly inert in either situation, namely, antimony and bismuth." Considering the explanation given, that the effects are due to the mechanical repulsion exerted by the heat in its passage from a good conductor to a bad one, the inertness of the two bodies mentioned presents a grave difficulty. Reflecting on the subject, the thought occurred to me, that if a mass of bismuth or antimony were cut so that the plane of most eminent cleavage might be vertical, the superior con-

ductivity which the mass probably possesses in the direction of the said cleavage might aid in the production of the vibrations. I cut such a piece from a mass of antimony, and fixed it in a vice, so that the horizontal edge on which the large brass rocker rested was perpendicular to the surfaces of principal cleavage. Loud and sustained vibrations were the consequence. I repeated the experiment in the case of bismuth with equal success; and, after a little practice, found that the precaution of cutting the substances in the manner just described was wholly unnecessary, and that tones could be obtained with facility, no matter what might be the direction in which the mass was cut.

We have thus proved antimony and bismuth to be active in one position at least; but antimony is active both as rocker and bearer. Two irregular masses, the one weighing about a pound and the other five pounds, were so filed down as to present suitable surfaces for rocking. Heated, and placed upon a flat mass of lead, both masses vibrated permanently. These experiments add their evidence to that already adduced against the third law; for antimony is a worse conductor than lead, and antimony is here the *hottest* metal.

These results appear to leave the theory of Professor Forbes without any foundation. One point only remains to be considered. Professor Faraday attributes the superiority of lead as a bearer to its great expansibility by heat, combined with its deficient conducting power. Against this view Professor Forbes argues in the ingenious manner already described. It cannot be denied that when the supporting metal is a good conductor, a greater quantity of heat will pass into it, during contact, than when it is a bad one. It cannot be denied that the greater the quantity of heat transmitted, the greater will be the expansion; and hence the conclusion seems unavoidable, that, if the vibration be due to expansion, both rocker and bearer, other things being equal, ought to possess the power of conduction in the highest possible degree.

Assuming then that the effects are produced by ordinary expansion, the argument of Professor Forbes, stated in its severe logical form, would be as follows :—

The greater the expansion the greater will be the effect; but,

The greater the conducting power the greater is the expansion : therefore,

The greater the conducting power the greater will be the effect.

This, to all appearance, is conclusive. A slight inadvertence, however, in the use of the term "expansion" appears to deprive the argument of much of its force. In the first proposition the term means expansion *in a vertical direction*; for if this be not

meant, the proposition would be untrue. In the second proposition, however, it is the *total expansion* that is referred to[*]. Now supposing the conductivity of the bearer to be infinite, that is to say, that the quantity of heat which it receives from the rocker during contact is instantaneously distributed equally throughout its entire mass, then, although the total expansion might be very great, there would be no *local* expansion at all, and therefore none of the effects in question. The expansion we require is a sudden elevation of the point where the rocker comes into contact with the bearer; and it is manifest that "a rapid communication with the interior" may, by suddenly withdrawing the heat from the point where it is communicated, almost extinguish the requisite elevation, and thus prevent the vibrations. This appears to be the precise reason why Professor Forbes has failed to obtain the numerous results described in the foregoing pages. His bearers were of such a form that the mass of matter, immediately surrounding the point of contact, quickly abstracted the heat communicated to that point, and thus destroyed the condition upon which the vibrations depend. The success of the experiments described in this memoir depends on the precaution, that the abstraction of heat was prevented, to some extent, by reducing the bearers to laminæ and mere spikes; and the fact that a thin edge gave a better tone than a thick one thus receives a full explanation. These considerations, I think, render it clear that the cause of the superiority of lead assigned by Professor Faraday is by no means an "oversight." On the other hand, it would not be safe to affirm generally, nor has it been affirmed by the philosopher last mentioned, that the less the conducting power the greater will be the effect. In the case of glass and earthenware the vibrations soon come to an end, for the requisite difference of temperature between rocker and bearer, as anticipated by Professor Forbes, soon ceases. Perfect non-conductibility would be just as inefficacious as perfect conductibility, and the region of practical results lies between these two extremes.

II. *On the Conductivity of Heated Glass for Electricity.* By Professor BUFF[†].

IT is known that glass strongly heated assumes the property of conducting electricity. This effect, although often observed, has not, however, since the time of Cavendish, been further investigated. A number of observations, some of them totally

new, which I have recently made in connexion with this subject, are, I think, worthy of publication.

Various kinds of glass exhibit, as everybody knows, great differences in respect to their power of insulation. Potash-glass is in general the best, and soda-glass the worst insulator. One of my hearers, Mr. Matthieson of London, showed me a description of glass, which, when covered on both sides with tinfoil, was completely incapable of assuming a charge. With very slight tension the two electricities crossed the glass and united with each other. By chemical analysis it was found that the glass in question was a soda-glass almost completely free from potash.

In my experiments I used, for the most part, glass vessels of very small thickness, in the form of chemical test tubes. This glass, although not difficult to fuse, proved at ordinary temperatures, and when dry, to be a good insulator. To ascertain the limits at which this property was lost, a tube 39 centims. long, 2 centims. exterior diameter, and 0·7 of a millim. in thickness, was filled with water to a height of about 6 centims. Into the water dipped a platinum wire, which remained in connexion with the positively charged conductor of a small electric machine. At a moderate heat no diminution of the insulating power was observed ; but when the temperature reached 40° or 50° C., on touching the exterior surface of the glass with the end of the helix of a very sensitive multiplying galvanometer, the other end of which was in connexion with the earth, a motion of the astatic needle of the instrument could be observed. When the temperature of the liquid approached its boiling-point, the flame which encompassed the tube began to waver, and was repelled from the surface of the glass. When the surface was discharged the wavering of the flame ceased, and at the same time the electro-meter pendulum placed upon the conductor of the machine sunk, although the machine was turned persistently, thus showing that the electricity passed speedily through the glass. By approaching the knuckle, sparks were obtained. A battery of 12 of Bunsen's elements was not sufficiently strong to force a current through the glass heated to 100° C.

To obtain a higher temperature, the water was poured out and its place taken by mercury. In heating the latter to its boiling-point, the glass became such a good conductor, that, by approaching the knuckle, sparks an inch in length could be drawn from it, the insulated flame of the spirit-lamp being at the same time blown aside by the repulsion. At a temperature of 220° or 230° C., the touching of the surface of the glass by a platinum wire was sufficient instantly to discharge the prime conductor. The current of a single Bunsen's element was sensibly conducted,

while an inch above the heated portion of the tube the glass retained its power of insulation.

The same tube partially filled with mercury was now caused to dip so far into a wider one, which also contained mercury, that the liquid metal stood at the same height within and without. The object was to increase the number of points of contact of the exterior surface, also of the tube with a good conductor. The mercury in the interior remained, as before, in connexion with the positive conductor of the machine, the mercury outside communicated with the end of the multiplying helix of a tangent compass, the other end of which was carried to the negative conductor, and also connected with the earth. In the liquid within the tube the bulb of a mercury thermometer was immersed. At the commencing temperature ($16°$ C.) the two metallic coatings could be strongly charged similar to a Leyden jar. Nevertheless a portion of the electricity seemed to pass through the glass; for the galvanometer, which was deflected at first by the outflow of the induced electricity, did not return to its position of equilibrium when the machine was turned in the most regular manner possible. The deflection was augmented when the temperature increased, while the phænomena of charge perceptibly diminished. At $80°$ C. the needle stood at $3°$; at $120°$, at $7°$; and finally, at $200°$ it stood at $7°·5$. A greater deflection than this could not be obtained by heating the mercury still more. When the glass was excluded from the circuit, and the contact was established by the metallic connexion of the prime conductor with the galvanometer wire, the deflection of the needle amounted likewise to $7·5$. It may be seen from this, that at temperatures over $100°$ almost the entire quantity of electricity was permitted to pass through the glass. The force necessary to overcome the resistance of the glass, the tension on the surface of the conductors, was of course not the same at the different temperatures. When the thermometer stood at $100°$, by the approximation of a small sphere of metal, sparks 3 millims. long might still be drawn from the conductor. At temperatures over $120°$ the passage of the sparks completely ceased. At $180°$ a very sensitive gold-leaf electrometer, unassisted by a condenser, scarcely showed a perceptible charge; and when the mercury approached its boiling-point, the same electrometer, even with the aid of the condenser, could no more be charged. These facts are perfectly in accordance with the diminution of the resistance of the glass by the augmentation of the temperature.

In consequence of the facility with which the glass permitted the passage of the electricity, it was possible to measure the resistance corresponding to each particular stage of temperature. The apparatus made use of for this purpose was that before de-

scribed; the closed end of the glass tube chosen for the experiments was almost perfectly spherical; the height of the interior column of mercury after the introduction of the thermometer was carefully measured, and before commencing the experiments, the surfaces of the metal within and without the tube were brought exactly to the same level.   By means of a cork which embraced the small tube, and fitted into the exterior one, the necessary position was easily secured.

The heating was effected by means of an Argand lamp, above the glass chimney of which the apparatus was placed and well insulated.   The heat generated by this lamp, the wick of which could be regulated with the greatest nicety by a suitable screw, was sufficiently great to elevate the mercury to its boiling temperature, while at the same time any lower temperature was more easily obtained than with the flame of the spirit-lamp.

The mercury was pure, dry, and besides this boiled in the apparatus, for the purpose of establishing the most intimate contact possible with the surface of the glass.   The connexions, as before, were established by platinum wires which dipped into the mercury.

This apparatus formed at the same time, and in connexion with a very delicate tangent compass, the circuit of a single element of Daniell.   The circuit closed by the tangent compass alone produced a deflection of 79°.

From some preliminary experiments it was observed, that during the passage of the current through the glass, the needle of the galvanometer assumed only a transitory deflection, which soon began slowly to approach zero, just as occurs when a liquid between two platinum plates is introduced into a circuit of small electromotive force.   In the latter case the effect is a consequence of polarization, that is, a consequence of the action of an opposing electric force which is gradually developed.   The glass apparatus also had evidently assumed a charge which acted in opposition to the electromotive force of the circuit ; when the voltaic element was excluded, and the coatings connected directly by a wire, a transitory deflection in the opposite direction to the original one was obtained.   In order to avoid as much as possible these disturbing influences, we must limit ourselves to the measurement of the first impulsion of the needle.   But as, during even this period, the opposing force, though in a very small degree, had begun to develope itself, the precaution was taken to conduct the current first from the interior of the glass to the exterior, and afterwards in the opposite direction, and to take the mean of the two deflections thus obtained.   The difference amounted in some cases only to a few tenths of a degree; but when the deflections were large, amounted to 1°·5 and even

to 2°. When greater differences exhibited themselves, the experiments wherein they were exhibited were rejected.

It was further ascertained by a particular series of experiments, that the first impulsions, as long as they did not exceed 40°, through multiplication by the factor 0·74, gave the corresponding permanent deflection with sufficient accuracy.

By means of the deflections ascertained in this manner, and the equations derived from the law of Ohm,

$$r \tan 79° = K \text{ and } (L+r) \tan \beta = K;$$

the resistance of the glass at different temperatures has been calculated. L denotes here the resistance sought, r the known resistance of the multiplying helix, which is so considerable that the resistance of a single pair of Daniell, or even several of them, disappears in comparison; β is the permanent deflection of the needle, K the force of an element of Daniell. From these equations we obtain

$$L = r \cdot \frac{\tan 79° - \tan \beta}{\tan \beta}.$$

The observations, and the resistances deduced from them, are contained in the following table:—

Temperature of mercury in tube.	First deflection α.	Permanent deflection $\beta = 0.74 \alpha$	L.
244 to 245	1·8	1·33	$215·2 . r$
255	3·3	2·44	$119·7 . r$
269 to 270	6·2	4·59	$63·1 . r$
283	12·0	8·88	$31·9 . r$
293 to 294	17·65	13·06	$21·2 . r$
296	19·15	14·15	$19·4 . r$
297	19·85	14·69	$18·6 . r$
300	21·8	16·13	$16·8 . r$
318	23·95	17·72	$15·1 . r$
320 to 323	25·0	18·5	$14·4 . r$
345 to 350	31·8	23·53	$10·8 . r$

The numbers in the last column show the ratio in which the resistance diminishes. If these numbers be chosen for ordinates, the temperatures, increasing in arithmetical progression, being chosen as abscissæ, we find that the curve thus formed falls very steeply at the commencement, and then approaches the abscissa asymptotically. From the numbers stated in the foregoing table the following one has been calculated by interpolation, in which, of course, the values which lie beyond the limits of observations can only be a probable approximation to the truth.

Temperature of the glass in Centigrade degrees.	Resistance.
200	$2582 \cdot 0 \cdot r$
250	$158 \cdot 3 \cdot r$
300	$16 \cdot 8 \cdot r$
350	$11 \cdot 8 \cdot r$
400	$8 \cdot 4 \cdot r$

Electric Polarization of Glass.

It has been already mentioned that the glass covered on both sides by mercury, after it has permitted of the passage of an electric current for some time, exhibits an independent electromotive force which acts in opposition to that of the current, and which may be exhibited both by the galvanometer and the gold-leaf electrometer. It was natural to regard this phænomenon as a kind of charge similar to that of the condenser. In harmony with this view was the fact, that the effect soon disappeared on connecting the two mercury coatings with a good conductor. A closer examination of the subject, however, does not appear to justify this assumption.

When the two coatings of a condensing apparatus are connected with the poles of a voltaic battery, it is known that a momentary contact is sufficient to obtain the maximum charge, and this maximum increases almost in the same proportion as the strength of the battery. The inner and outer coatings of the glass tube behaved exactly similar as long as low temperatures were made use of; when, however, the temperatures were high, very remarkable divergences appeared, as the following experiments will show.

One of the mercury coatings was connected with the positive, and the other with the negative pole of a Bunsen's battery of three elements. At a temperature of 16° C. a charge was obtained, which, after the exclusion of the battery, produced a divergence of one inch of the gold-leaf electrometer. This corresponded to the maximum action of the three elements. At a temperature of 50° C. this divergence amounted under the same circumstances to 4 lines, at 100° to only 1 line, and at 300° sunk to less than half a line, although a small divergence was still distinctly perceptible. Notwithstanding this great diminution in the strength of the charge in the case of the heated glass, a brief contact with the poles of the battery was by no means sufficient to produce the maximum charge; for this purpose, on the contrary, a contact of several minutes was necessary. When the charge was once imparted, it was much more persistent than that obtained at ordinary temperatures by momentary contact.

It was sufficiently powerful to deflect the needle of the tangent compass, after the removal of the battery, when the coatings were connected with the ends of the helix of the instrument. A deflection increasing with the temperature of the glass was thus remarked, although the tension must have been diminished at the higher temperatures. At ordinary temperatures, where the tension was greatest, no action whatever was produced upon the needle.

This decrease of the intensity of the charge, and the simultaneous increase of the quantity of the electricity conducted from the surfaces of the glass, is quite irreconcileable with the notion of a charge similar to that of the Leyden jar, or a condensation of the electricity on the opposite surfaces of the glass. The effect is a natural and necessary consequence of the assumption, that the glass softened by heating, like a liquid conductor under the same circumstances, has endured a polarization, that is, a chemical alteration of its surface.

When the mercury on both sides of the glass had reached its boiling-point, a polarization could be obtained of sufficient strength to produce a deflection of 50° or 60°. This action could be produced by the polarizing powers of a battery of three or four Bunsen's elements, but in a much shorter time with a more powerful battery.

When the polarization is strongly developed, it persists with great obstinacy, and in all probability the change on which it depends penetrates to some depth into the glass. The following experiment will illustrate this. The glass tube, polarized when the mercury was at its boiling-point, was permitted to cool and then freed from its metallic coatings. The internal and external surfaces were cleansed with nitric acid, washed with distilled water, and dried over the flame of a spirit-lamp. When afterwards the glass tube was immersed in mercury, as before, and partially filled with the same liquid metal, on heating the mercury again to its boiling-point, and connecting the coatings and the galvanometer wire, a deflection of 30° in the same direction as that of the former polarization was obtained, the needle sinking slowly afterwards towards zero*.

Electric Batteries in which glass plays the part of the moist conductor.

When in the mercury of the tube, or in the mass which surrounded the latter, a quantity of zinc was dissolved, the electric motion was in a determinate direction, and passed in a positive direction (through the connecting wire?) from the pure mercury. This might be shown either electroscopically or by the galvano-

* See Kohlrausch in the two preceding Numbers of Phil. Mag.—Eds.

meter. A stronger electroscopic arrangement was obtained when the tube contained mercury, or a liquid amalgam of zinc, and was surrounded by peroxide of manganese or powdered charcoal pressed firmly together. The condenser received from the mercury or the amalgam negative, and from the peroxide of manganese or the coal positive electricity. The current passed, therefore, from the amalgam through the glass to the peroxide, and thence through the connecting wire, just as it would proceed had water been used instead of glass. The charge was augmented considerably by heating the apparatus, and it was found that such an arrangement was capable of exhibiting phænomena of polarization similar to those already described.

Piles can also be formed without difficulty in which the liquid conductor is superseded by glass. Brass discs gilt, clean discs of zinc and thin plates of glass, were placed one over the other in the same order. To the first gilt disc, and also to the last which covered the tenth plate of glass, platinum wires were attached; the pile thus formed was only 4 centimetres high. The plates were so pressed together that they might be exposed to the hot current of an Argand lamp. When time was permitted to this little pile, the condenser at ordinary temperatures could be charged to a tension of three or four lines divergence of the gold-leaf electrometer. When the discs were heated, a few seconds' connexion with the condenser produced a divergence of at least fourteen lines. Such a pile constructed in 1853, and often used, did not in five months lose in the smallest degree its primitive electroscopic force.

III. *On the Rotation of the Pendulum.* By Alfred Day, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,   Westbury on Trym, near Bristol, May 23, 1854.

HAVING been one of the parties who, in an early stage of the inquiry, proposed a simple original solution of the phænomena of the rotation of the pendulum, which is now commonly adopted without acknowledgement, and which has been again and again repeated both here and in America, I am desirous of correcting some unsatisfactory representations which still find their way into print in works of a respectable class. I refer to the account given of the problem in a manual of the physical sciences by Dr. Golding Bird and Mr. Brooke, where the correct explanation is also given, being exactly the same as my own, even to the form in which the trigonometrical relation

is expressed, the same fraction, $\dfrac{\cos.\lambda}{\cot.\lambda}$, occurring in both. But this explanation is accompanied, or rather prefaced, by some remarks, which represent the problem, first, as having excited much more attention than it deserved; next, that the rotation of the pendulum is not an immediate effect of the earth's rotation; and thirdly, that the rotation of the pendulum is only an *apparent* rotation. Now on this I cannot help remarking, that the problem would have been most interesting, and deserving all the scientific examination it received, if only as a purely speculative or abstract one; and it was very remarkable that the precise case had never been discussed. As it was, it took mathematicians by surprise, baffled not a few respectable professors of the science, and was wrongly apprehended by several analysts of considerable eminence.   None of the analytical solutions, if such they can be termed, that have from time to time appeared in this country, have established anything more than is done with far greater clearness and simplicity by the explanation given in the work referred to; and the whole thing, notwithstanding its great interest and novelty, has sunk into temporary contempt and neglect, as if the scientific republic was rather ashamed of having allowed so much discussion and so much algebra to be wasted on a thing so readily demonstrable by a person tolerably conversant with Euclid only, or, to say the least, with Euclid and the first elements of plane trigonometry.   By way of retaliation, as there is no glory to be got out of it, the problem is now never mentioned, and a large body of people at this moment look upon the whole thing as tacitly given up, notwithstanding that the exact experiments of Mr. Bunt, certainly undertaken with no bias in favour of bringing out the precise result, have proved it, in a great variety of trials, to be true in practice, which is by no means necessary to establish the truth of the theoretic reasoning.   It is not however as a speculative case that the problem is of the highest interest, but as an immediate proof of the earth's motion round its axis; and the discovery was hailed as important chiefly on this ground by the members of the French Institute.   It is true you do not see the earth's rotation any more than before, nor does the pendulum move away at a rate necessarily corresponding with that in which the former is performed.   I may state the real value of the illustration thus.   It is only within a comparatively recent period of the world's history that the apparent motion of the heavens has been finally reconciled with the supposition of a motion in the spectator on the earth's surface, and not in them; and while no objection can be raised against the adequacy of this supposition to account for what is witnessed, there are a hundred

reasons why it should be adopted implicitly, without one to be urged on behalf of the ancient hypothesis which makes the earth stand still. We have heard indeed of incredulity on this point, but it must be confined to those who, with their systems of philosophy or theology, stand as still as the earth on their theory; and to offer a serious refutation would be loss of time, without the hope of bettering the state of their convictions. Notwithstanding however that the mass of men are fully agreed on this question, and that the great conspiracy of arguments tends to render the conclusion in the highest degree probable, and to the philosopher certain, it cannot be denied that a diurnal rotation of the heavens would produce effects such as those ordinarily observed, and that the reasons which lie against it are not those which can be easily understood by persons unskilled in physical demonstration. We are glad then of a new fact, which, though it does not really make the rotation of the earth more a sensible phænomenon than before, shows that a relative motion exists between the earth and other independent planes, in which the same positions do not necessarily recur after twenty-four hours, and which are in no way affected by any rotation of the sidereal heavens, and yet are wholly explicable on the hypothesis of the earth's rotation round its axis in twenty-four hours, and on no other. To a person who views the pendulum experiment, the rotation of the plane of its vibration is readily and speedily manifest; and because this may be shown to depend on the rotation of the earth, it is not unnaturally designated a making the earth's rotation visible. We do not, however, apart from reasoning, attribute the effect observed to the motion of the earth, any more than we do the motion of the stars about us to that cause; for what we appear to see is strictly a motion of the plane of vibration round the zenith as a pole. It may suit the purpose of popular explanation to say that the plane of the pendulum's vibration remains unchanged while the earth rotates under it; but a little consideration shows that this is not strictly true, and the conception of what does take place becomes more difficult than in the case of the apparent motion of the heavens, since the pendulum being carried at its point of suspension with the earth, appears to be one with it. But though the earth's diurnal rotation is less directly proveable in this way, it is far more conclusively so than it is by the motion of the stars. In these last, the motion having existed from the commencement of all things, might have been originally impressed as well as that of the earth; but with the pendulum set in motion by human agency, and continuing to vibrate without solicitation to the right hand or left, on the supposition of the earth being at rest, no such apparent rotation could be brought about on any known physical

law. It is therefore the earth that moves, not at the rate of the pendulum nor round the pendulum-wire when at rest as an axis, though this it might do if this were consistent with the observed motions of the stars ; but round another axis pointing to another star, and at a rate coinciding with the apparent rate of the sidereal revolutions. It is therefore a sensible proof of another kind than any previously known to exist, and of directly sensible proofs measured by the eye in short intervals of time, the most conclusive in its kind. The value of the whole argument may be thus stated. Either the heavens or the earth rotate once in twenty-four hours, or both move by a joint motion away from each other equivalent to this in amount ; and either of these conditions is abstractly possible, so that no conclusion can be drawn. In this latitude the pendulum rotates in about thirty hours, or the earth in twenty-four ; and as there can be no assignable reason why the former should do this without a cause, the rotation of the earth in twenty-four is a fact for the rational being, since it is clear that no supposition of the observed motion being partly in the earth and partly in the heavens will fulfill the second set of conditions. The experiment of the pendulum is therefore very correctly designated as a making visible the earth's rotation, though hardly in the sense popularly attached to it ; and its importance cannot, we think, be overrated. So much then for the second objection ; and I shall now proceed to show that the rotation is to all intents and purposes real, and not merely apparent on the part of the vibrating plane. I have elsewhere, in a published diagram, shown that the pendulum's actual rotation is the resultant of two sets of continually-exerted forces, one tending to keep the pendulum swaying parallel to itself, the other to make its point of suspension and the line which the pendulum would occupy if at rest rotate obliquely to the axis of the two circles described by the point of suspension and the ball at the mean of the oscillation. Shortly after the publication in question, I constructed a simple mode of illustrating the fact. Cut out two circular discs of card, and make an equal number of cogs or teeth in the rim of each. Paste one of these on a larger circular disc and concentric with it, and cut both through along any radius from the circumference to the centre. If now we draw a diameter on the other toothed disc, and cause it to travel round by placing the teeth of the two discs in one another in the manner of ordinary wheels when acting on each other, but so that the larger disc shall remain motionless, we shall find that this line will have rotated once for one entire revolution of the moving wheel round the circumference of the fixed one. If now we coil up the large disc with its attached rack into the shape of a cone, so as to hide some of the teeth,

our index-line will not rotate once during the revolution of its disc over the entire surface of the rack on the conical surface; and the more acute the cone is the smaller the amount of rotation. This is not only analogous to the case of the pendulum, but it is exactly the same in amount, when the plane sectional angle of the cone is altered proportionally to the latitude of the place where the pendulum is set swinging.

Great confusion has arisen in many minds with reference to this problem from not separating two things, a free and a constrained and conditioned motion, and not distinguishing what is real from what is apparent. Thus if a cone be set revolving on its axis, a fixed straight line on the surface of the cone will, after one entire revolution, be in the same spot, and we say of it that it has not rotated round any point in itself; but yet if a plane be conceived to pass through the line in its first position and the axis of the cone, and the projection of the line after a quarter revolution falling perpendicular on this plane be drawn, the two lines will cut one another at an angle equal to half the plane sectional angle of the apex of the cone, and in half a revolution this angle will be doubled. The partial rotation however in the first half of its course, which reaches its maximum in half a revolution of the cone, is in fact retrograde during the second half, and at the completion of the whole period all things are as they were. The whole of this is a constrained motion, during which every point in the line moves in a circle, but every point with a velocity different from that of its neighbour and in a different circle. This is totally different from the case of a line occupying at each infinitesimally-consecutive instant a position parallel to itself, and thus having all its points moving uniformly in equal circles, or constrained to keep to the surface of the revolving cone, so as to cross always a given point on it, or, in other words, moving at the same rate as the cone. It has been sufficiently and irrefragably demonstrated, that while the cone continues to revolve in the same direction, the line so circumstanced would constantly rotate in one direction round that point which it always crosses; and that though this result is obtained by the tendency of the line to keep parallel to itself, coupled with the constraint applied to it, it will not, after a complete revolution of the cone on its axis, bring the line into parallelism with its first position, but only after the lapse of a longer period. Hence the rotation is real, not merely *apparent*; and were there no apparent rotation, there must have been a real retrograde force of rotation applied to the plane of oscillation, so as to have obliterated all the advance we actually see in practice. I have met with other gentlemen of mathematical education and reputation, who have persisted in calling this rotation an apparent one,

which it certainly is, and something more*. When we talk of the apparent rotation of the heavens, we mean that they do not revolve, but only the earth, in twenty-four hours all things being replaced. If the earth really revolves in twenty-four hours, it is certain that the oscillation of the pendulum is not in the same direction, and consequently it has rotated, unless any one is absurd enough to maintain that the rotation of the earth itself varies its period at different latitudes.

I am, Sir,

Your obedient Servant,

ALFRED DAY.

IV. *On the probable Nature of the Sun's Body and on a new Process for rendering Collodion more sensitive.* By THOMAS WOODS, *M.D.*†

THE physical nature of the sun's body, whether it be solid, gaseous, or both, is a matter of doubt to philosophers. The peculiar appearance of the spots and the changes they undergo, lead to the supposition that a gaseous envelope surrounds whatever may be the interior; and Arago's determination, that the sun's direct light is not polarized, renders it probable that the envelope is flame.

I have lately made some photographic experiments, which may tend further to the establishment of this opinion. It has been long known that the light proceeding from the centre of the sun is more intense than that from the edge. I have taken several pictures of the sun in a camera obscura, by means of a photographic process, exposing the prepared surface of the plate to be acted on for different periods of time. For instance, the camera being set, and the prepared plate in focus, I have allowed the picture of the sun to fall on it for as short a time as it was possible to uncover and cover again the aperture. I then opened the aperture again for a somewhat longer period, having first moved the plate so that another part of it would be acted on. I caused the picture then to fall on another portion of the plate for a longer time, and so on, taking on the same prepared surface six or eight pictures, each the result of a different length of ex-

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* There is no rotatory tendency in the plane of oscillation, excepting in so far as this is produced as a result of the earth's constraint on the point of suspension, in other words, the rotatory effect is due to the earth. In this sense it is the earth's rotation made visible though the angular motion is slower than that of the earth; and the case may be compared to the motion of the box containing the main-spring of a watch, as evidenced by the motions of the hands.

† Communicated by the Author.

posure. The examination of these pictures showed that they were of different *sizes,* the smallest being produced by the shortest exposure; and that they increased in extent with the length of time the aperture was open up to a certain size. The centre of the picture was apparently intensely acted on, as it had the appearance of being what photographers called "burnt." And this deep spot was surrounded by a ring of light not so darkly marked. The "burnt" centre increased in size, not in depth of intensity, with increased length of exposure; the ring about it also increased, but not in proportion to the enlargement of the centre. A piece of red glass placed before the aperture of the camera renders the sun's action less powerful, and allows the pictures to be taken less rapidly. These experiments are a further proof that the light from the centre of the sun acts more energetically than that from the edges; the latter requiring a longer time to produce as much effect as the former on a photographic surface.

I thought that these experiments might furnish a ground for obtaining evidence of the probable nature of the sun's envelope. I determined to try whether *flame* would affect a sensitive plate after a manner similar to the sun; and if so, whether a *solid* body producing light would differ in its action. I therefore exposed a prepared surface in a camera in the focus of a lighted candle, and also of a gas-jet. In both cases the action was exactly similar to that of the sun, but more marked, as to variety of extent, in the size of the pictures produced, because the light was not so powerful, thereby allowing more leisure in the manipulation. The picture of the flames in one second made a slight impression, in two seconds the impression increased in size and apparent depth of action, and so on, up to 15 seconds, when the picture produced was about three times the size of that taken in one second in the case of the gas-jet. In numerous experiments I have made with flames, caused to burn steadily lest the wavering might influence the result, I have always found that their action on the plate was similar to that of the rays from the sun's disc, viz. an increased extent of picture for an increased period of exposure.

It now remained to try what effect a solid body giving out light—not reflecting it—would produce. It was not easy to find a means of heating a solid body sufficiently high to get a light capable of acting on a sensitive plate. I tried iron heated to whiteness, and platina in the flame of a gas-jet, but neither affected the plate in a camera. The lime-light, however, acted well. A piece of lime acted on by the oxy-hydrogen blowpipe was rendered luminous, and a picture of it thrown on the prepared surface by the camera. In one second a deeply-marked

image was produced; and the *size* of the picture of the solid was not influenced by the length of time of exposure. I at first thought a very slight difference of size was apparent in pictures produced by largely different lengths of time the plate was acted on, but I found on examination that the appearance was produced by the gases employed to heat the lime. I mention the circumstance in order to guard others from the mistake.

Taking into consideration all the experiments I have made, I have no doubt that the light from the centre of *flame* acts more energetically than that from the edge on a surface capable of receiving its impression; and that light from a luminous solid body acts equally powerfully from its centre or its edges; and therefore conclude that, as the sun affects a sensitive plate similarly with flame, it is probable its light-producing portion is of a similar nature.

In the experiment I have just spoken of, I used a sensitive surface prepared according to a formula which I am anxious to publish, as I believe it will be found more sensitive, and perhaps more easily manageable than any other hitherto known. I have by means of it taken a very good picture of a building, on a bright day, in as short a time as it was possible to uncover and cover again the aperture of the camera with the hand. The length of the focus of the lens was 6 inches, its aperture $\frac{3}{8}$ths of an inch in diameter. The process differs from the usual collodion one in substituting a mixture of iodide and chloride of iron for iodide of potassium, and using collodion having in solution some common salt. In 1844 I first introduced the iodide of iron as a photographic agent. I found it at that time the most sensitive I could procure, and since then, whether in processes on paper, or in albumen or collodion on glass plates, I have always succeeded with it better than with any other, both as to rapidity and facility of use. Mr. Fox Talbot has found it sufficiently sensitive when employed with Mr. Hunt's discovery of sulphate of iron, to produce instantaneous results. The details of the process are as follow :—

Take of		
Sulphate of iron	40	grains.
Iodide of potassium	24	grains.
Common salt	6	grains.
Spirits of wine or alcohol	2	oz.
Æther	2	drachms.
Strong water of ammonia	3	drops.

Powder the salts and mix them well together, add the alcohol and æther, and finally the ammonia. Allow the precipitate to subside. For preparing the plate, mix one part of the clear solution with three parts of collodion, to which has been added

a saturated solution of common salt in the proportion of one fluid drachm of the salt solution to four ounces of collodion. Spread on the glass plate in the usual way and immerse it for one minute to one minute and a half in a neutral solution of nitrate of silver, 30 grains to the ounce. Develope the picture with a solution of sulphate of iron one scruple to the ounce of water; and finally fix with the hyposulphite of soda. A very beautiful picture may also be obtained by using the developing solution of sulphate of iron, of the strength of 20 or 30 grains to 4 ounces of water, and adding to the hyposulphite wash strong water of ammonia, in the proportion of 20 drops of the latter to 6 or 8 ounces of the former. The iron solution should be well washed off previously to putting the plate in the ammonia and hyposulphite. By this process I have obtained most exquisite pictures in very short spaces of time. In many cases the light parts of the pictures are pure silver, forming a good mirror.

Alkaline reaction in the bath or the collodion causes cloudiness, which may, however, be remedied by a corresponding amount of nitric acid. If a bottle of strong ammonia be left open in the room where the plate is prepared, cloudiness will be produced. I brought for a few minutes a dish containing a solution of hyposulphite of soda, to which had been added a small portion of hydrosulphuret of ammonia, into the room in which I prepared a plate, and for some hours after I could not get a picture without cloudiness. By thoroughly ventilating the room I got rid of the annoyance. It would therefore be better if the ammonia be added to the hyposulphite solution, as recommended above, to keep it at a safe distance from the other materials. I have also found that filtering the caustic solution through the red-coloured blotting-paper is sufficient to produce alkaline reaction and cloudiness.

Parsonstown, June, 1854.

V. *On* Professor Challis's *new Theorems relating to the Moon's Orbit.* By J. C. ADAMS, *Esq., F.R.S., Fellow of Pembroke College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the June Number of your valuable Journal, Professor Challis calls attention to some circumstances connected with his withdrawal of a paper, relating to the moon's motion, which he had communicated to the Cambridge Philosophical Society, and of the principal results of which he had given an account in your Number for April (p. 278).

Professor Challis mentions that one of the reporters, whose

unfavourable judgement led to this withdrawal, had of his own accord communicated to him some of the reasons on which this judgement was based. Professor Challis, however, thinks these reasons to be very unsatisfactory, and consequently invites the reporter to discuss with him the questions on which they are at issue, in the pages of the Philosophical Magazine.

As I am the reporter thus referred to, I beg that you will allow me to state some reasons which appear to me sufficient to prove, beyond a doubt, that the principal conclusions of Professor Challis's paper are erroneous, in order that he may have the opportunity, which he desires, of replying publicly to my objections*. At the same time, I must decline to enter into any prolonged controversy on the subject, submitting with confidence what I have now to say to those who are competent to form a judgement respecting it.

The principal results of Professor Challis's paper are embodied in two theorems, which, as already stated, form the subject of an article in the Philosophical Magazine for April last. As my main objections to the paper relate to these theorems, I shall confine my observations almost entirely to the article in question.

It will be convenient, however, to make a few preliminary remarks on the nature of the process usually followed in the lunar theory. Professor Challis objects to the *logic* of this process, on the ground that the introduction of the quantities usually denoted by c and g into the first approximation to the moon's motion is only suggested by observation. He therefore considers the results of the ordinary process to be *hypothetical,* until they are confirmed by observation.

But surely the *sufficient* and the *only* test of the correctness of any solution is, that it should satisfy the differential equations of motion at the same time that it contains the proper number of arbitrary constants to fulfill any given initial conditions.

Any process which does this, no matter how it may be *suggested* to us, must be logical; and if the results obtained by it should not agree with observation, the conclusion would be that the law of gravitation, which was assumed in forming the original differential equations, is not really the law of nature.

If we begin with the supposition that the moon's orbit is an *immoveable ellipse,* the differential equations cannot be satisfied, without adding, to the first approximate expressions for the moon's coordinates, quantities which are capable of indefinite increase; and this proves, as is stated by Professor Challis, that an immoveable ellipse is not, or rather does not continue to be, an approximation to the real orbit.

* It may be proper to mention that the opinion of the other reporter on the paper perfectly agreed with my own.

But if we introduce the quantities usually denoted by c and g, having assigned values slightly differing from unity, which amounts to supposing the apse and node to have certain mean motions, we find that the differential equations are satisfied by adding to the first approximate expressions for the moon's coordinates, terms, which always remain *small*; and we thus know that our first approximation was a good one, and that the *true* and the *only true* solution of the differential equations has been obtained.

On the other hand, no solution can be a true one, which does not contain the proper number of arbitrary constants; and any person who asserts that one of the constants usually considered *arbitrary* is not so, is bound to show by what other really arbitrary constant the former is replaced.

I will now proceed to consider Professor Challis's two theorems, which are thus enunciated by him.

Theorem I. All small quantities of the second order being taken into account, the relation between the radius-vector and the time in the moon's orbit is the same as that in an orbit described by a body acted upon by a force tending to a fixed centre.

Theorem II. The eccentricity of the moon's orbit is a function of the ratio of her periodic time to the earth's periodic time, and the first approximation to its value is that ratio divided by the square root of 2.

I will endeavour, in the first place, to show that these theorems cannot possibly be true; and secondly, to point out the fallacies in the argument by which Professor Challis attempts to establish them.

The problem will be simplified by supposing the moon to move in the plane of the ecliptic, and the earth's orbit to be a circle. On these suppositions, Professor Challis's fundamental equations become

$$\frac{d^2x}{dt^2} = -\frac{\mu x}{r^3} + \frac{m'x}{2a'^3} + \frac{3m'r}{2a'^3}\cos(\theta - 2\overline{n't+\epsilon'})$$

$$\frac{d^2y}{dt^2} = -\frac{\mu y}{r^3} + \frac{m'y}{2a'^3} - \frac{3m'r_s}{2a'^3}\sin(\theta - 2\overline{n't+\epsilon'}).$$

Multiply these equations by y and x respectively, and subtract the results; and again multiply by x and y, and add the results together; thus we obtain, after expressing x and y by means of polar coordinates,

$$\frac{d}{dt}\left(r^2\frac{d\theta}{dt}\right) = -\frac{3m'r^2}{2a'^3}\sin(2\theta - 2\overline{n't+\epsilon'}). \quad \dots \quad (1)$$

$$\frac{d^2r}{dt^2} - r\left(\frac{d\theta}{dt}\right)^2 = -\frac{\mu}{r^2} + \frac{m'r}{2a'^3} + \frac{3m'r}{2a'^3}\cos(2\theta - 2\overline{n't+\epsilon'}). \quad (2)$$

Now these equations, which are equivalent to the former, are satisfied to terms of the second order inclusive by putting

$$r = a\left\{1 - \frac{m^2}{6} + \frac{1}{2}e^2 - e\cos(cnt+\epsilon-\varpi) - \frac{1}{2}e^2\cos 2(cnt+\epsilon-\varpi)\right.$$
$$- m^2\cos(\overline{2nt+\epsilon} - \overline{2n't+\epsilon'})$$
$$\left. - \frac{15}{8}me\cos(\overline{2nt+\epsilon} - \overline{2n't+\epsilon'} - \overline{cnt+\epsilon-\varpi})\right\}$$

$$\theta = nt + \epsilon + 2e\sin(cnt+\epsilon-\varpi) + \frac{5}{4}e^2\sin 2(cnt+\epsilon-\varpi)$$
$$+ \frac{11}{8}m^2\sin(\overline{2nt+\epsilon} - \overline{2n't+\epsilon'})$$
$$+ \frac{15}{4}me\sin(\overline{2nt+\epsilon} - \overline{2n't+\epsilon'} - \overline{cnt+\epsilon-\varpi}),$$

where $n^2 = \dfrac{\mu}{a^3}, \quad n'^2 = \dfrac{m'}{a'^3}, \quad m = \dfrac{n'}{n}, \quad c = 1 - \dfrac{3}{4}m^2,$

and a, ϵ, e, and ϖ are the four arbitrary constants required by the complete solution.

The fact that the differential equations are satisfied by these expressions for r and θ, whatever be the value of e, is quite sufficient to show that Professor Challis is mistaken in restricting e to one particular value.

The terms of the *second order* in the value of r, which depend on the arguments

$$\overline{2nt+\epsilon} - \overline{2n't+\epsilon'} \text{ and } \overline{2nt+\epsilon} - \overline{2n't+\epsilon'} - \overline{cnt+\epsilon-\varpi},$$

and which constitute the well-known inequalities called the "variation" and the "evection," prove the incorrectness of Professor Challis's Theorem I.; since in an orbit described by a body acted on by a force tending to a fixed centre, and varying, as Professor Challis supposes, as some function of the distance, the expression for the radius-vector in terms of the time cannot possibly contain any terms dependent on the *sun's longitude*.

I now come to consider the reasoning by which Professor Challis arrives at his theorems. All this reasoning is based on his equation

$$\left(\frac{dr}{dt}\right)^2 + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{m'r^2}{2a'^3} + \mathrm{C} = 0, \quad . \quad . \quad . \quad (\mathrm{C})$$

the truth of which, he says, cannot be contested. In speaking of the *truth* of this equation, Professor Challis cannot mean that it is anything more than an *approximation* to the truth, since in forming it he avowedly neglects all quantities of orders superior to the second.

Now what I assert is, *first*, that the *degree of approximation* attained by the equation (C) is not sufficient to justify Professor

Challis in inferring Theorem I. from it; and *secondly*, that Theorem II. does not follow from that equation at all.

To prove the first of these assertions, I remark that the equation (C) gives an approximate value of $\left(\dfrac{dr}{dt}\right)^2$ in terms of r, but that it does not profess to include terms of the third order. Now $\dfrac{dr}{dt}$ is itself a quantity of the first order, and consequently an error of the third order in $\left(\dfrac{dr}{dt}\right)^2$ leads to one of the second order in $\dfrac{dr}{dt}$, and therefore to one of the same order in the value of r expressed in terms of t. Hence Professor Challis is not entitled to infer that the relation between the radius-vector and the time in the moon's orbit is the same, to quantities of the second order, as that which would be given by the equation (C).

We may test the degree of accuracy to be attained by the use of this equation in the following manner.

By differentiation, the constant C disappears, and the resulting equation becomes divisible by $\dfrac{dr}{dt}$; dividing out, we obtain

$$\frac{d^2r}{dt^2} - \frac{h^2}{r^3} + \frac{\mu}{r^2} - \frac{m'r}{2a'^3} = 0.$$

This is a strict deduction from Professor Challis's equation; we will now obtain directly from the equations of motion given above, an expression to be compared with it.

Integrating equation (1), and putting, with Professor Challis, $nt + \epsilon$ for θ, and a for r in the term of the second order, we find

$$r^2 \frac{d\theta}{dt} = h + \frac{3}{4}\frac{m'}{a'^3}\frac{a^2}{n} \cos (\overline{2nt+\epsilon} - \overline{2n't+\epsilon'}).$$

The value of the constant h, expressed in terms of the system of constants before used, is

$$h = na^2\left(1 - \frac{m^2}{3} - \frac{e^2}{2}\right).$$

Hence

$$r^4\left(\frac{d\theta}{dt}\right)^2 = h^2 + \frac{3}{2}\frac{m'}{a'^3}a^4 \cos (\overline{2nt+\epsilon} - \overline{2n't+\epsilon'}),$$

and

$$r\left(\frac{d\theta}{dt}\right)^2 = \frac{h^2}{r^3} + \frac{3}{2}\frac{m'}{a'^3}a \cos (\overline{2nt+\epsilon} - \overline{2n't+\epsilon'}),$$

putting, as before, a for r in the small term. Substituting this value of $r\left(\dfrac{d\theta}{dt}\right)^2$ in equation (2), we find

$$\frac{d^2r}{dt^2} - \frac{h^2}{r^3} + \frac{\mu}{r^2} - \frac{m'r}{2a'^3} - 3\frac{m'}{a'^3}a \cos (\overline{2nt+\epsilon} - \overline{2n't+\epsilon'}) = 0.$$

The equation above deduced from Professor Challis's differs from this by the omission of the last term, which gives rise to the *variation* inequality. In order to find the *evection*, which is also an inequality of the second order, it would be necessary to carry the approximation one step still further than we have here done.

This shows how unfitted equation (C) is for giving any accurate information respecting the moon's orbit.

As a matter of fact, it may be observed that this equation would make the moon's apsidal distances to be *constant*. A simple inspection of the calculated values of the moon's horizontal parallax, given in the Nautical Almanac, is sufficient to show how far this is from the truth.

I now proceed to make good my second assertion, viz. that Professor Challis's Theorem II. cannot be inferred from his equation (C). The process by which he attempts so to infer it is of the following nature. He first finds that a method, apparently legitimate, of treating the equation (C) leads to a difficulty. To get rid of this difficulty, he makes the strange supposition that the equation (C) contains the disturbing force as a factor, and then tries to show that, in order that this condition may be satisfied, the arbitrary constants h and C must have a certain relation to each other, from which it would immediately follow that the eccentricity must have the value assigned to it in Theorem II.

Now it is remarkable that every one of the steps of this process is unwarranted. The difficulty to which Professor Challis is led is purely imaginary; the supposition that the equation (C) contains the disturbing force as a factor is wholly unsupported by any proof; and even if that supposition were well founded, it would not follow that the constants h and C must have the relation assigned to them by Professor Challis.

The supposed difficulty is founded on the inference at the bottom of p. 280 of Professor Challis's paper, " Hence we must conclude that the mean distance and mean periodic time in this approximation to the moon's orbit are the same as those in an elliptic orbit described by the action of the central force $\frac{\mu}{r^2}$." But this is not a correct conclusion: if h and C be supposed to have the same values in equation (C) and in that obtained from it by putting a for r in the small term, the values of the mean distances in the two cases would not be the same, but would differ by a quantity of the second order.

This may be readily shown in the following manner.

At the apsides $\frac{dr}{dt} = 0$, and therefore the equation (C) gives the

following equation for finding the apsidal distances,

$$h^2 - 2\mu r + Cr^2 - \frac{m'}{2a'^3} r^4 = 0.$$

Now if a be the mean distance, and e the eccentricity, the apsidal distances are $a(1+e)$ and $a(1-e)$.

Substituting these values for r in the above equation, and developing the small term to quantities of the fourth order, we obtain

$$h^2 - 2\mu a(1+e) + Ca^2(1+2e+e^2) - \frac{m'}{2a'^3} a^4(1+4e+6e^2) = 0$$

and

$$h^2 - 2\mu a(1-e) + Ca^2(1-2e+e^2) - \frac{m'}{2a'^3} a^4(1-4e+6e^2) = 0;$$

whence it follows that

$$h^2 - 2\mu a + Ca^2(1+e^2) - \frac{m'}{2a'^3} a^4(1+6e^2) = 0$$

and

$$\mu a - Ca^2 + \frac{m'}{a'^3} a^4 = 0.$$

These equations give the relations between the arbitrary constants h and C, and the new constants a and e by which the former may be replaced.

From the second of them, we find

$$a = \frac{\mu}{C} + \frac{m'}{a'^3} \frac{a^3}{C};$$

or, putting for a in the small term its first approximate value $\frac{\mu}{C}$,

$$a = \frac{\mu}{C} + \frac{m'}{a'^3} \frac{\mu^3}{C^4},$$

which agrees with Professor Challis's expression in p. 281.

Now apply a similar process to the equation

$$\left(\frac{dr}{dt}\right)^2 + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{m'a^2}{2a'^3} + C = 0,$$

which differs from the equation (C) in having a put for r in the small term. In this case, we find

$$h^2 - 2\mu a + Ca^2(1+e^2) - \frac{m'}{2a'^3} a^4(1+e^2) = 0$$

and

$$\mu a - Ca^2 + \frac{m'}{2a'^3} a^4 = 0;$$

from the latter of which equations it follows that

$$a = \frac{\mu}{C} + \frac{m'}{2a'^3} \frac{a^3}{C},$$

or

$$a = \frac{\mu}{C} + \frac{m'}{2a'^3}\frac{\mu^3}{C^4}$$

to the same degree of approximation as before.

Hence we see that the values of a, in the two cases supposed, differ by a quantity of the second order. Consequently the difficulty into which Professor Challis is led by the conclusion that these values are the same, disappears, and the solution of the difficulty with it.

But even if we were to suppose, with Professor Challis, that the equation (C) contains the disturbing force as a factor (of which, as already remarked, no proof whatever is given), it would not follow, as is inferred by him, that h^2C must be equal to μ^2. On the contrary, it is evident that the required condition would be satisfied if h^2C differed from μ^2 by any quantity involving the disturbing force as a factor; whence it would follow that e must be *some* function, indeed, of the disturbing force, but it could not be decided *what* function.

Professor Challis attempts to find the relation between r and t by direct integration of the equation

$$dt = \frac{-dr}{\sqrt{-C - \frac{h^2}{r^2} + \frac{2\mu}{r} + \frac{m'r^2}{2a'^3}}}.$$

Now it may be remarked that $\left(\frac{dr}{dt}\right)^2$ is a small quantity of the second order which vanishes twice in each revolution, and that the difference between the complete value of $\left(\frac{dr}{dt}\right)^2$ and the approximate value

$$-C - \frac{h^2}{r^2} + \frac{2\mu}{r} + \frac{m'r^2}{2a'^3},$$

which is used instead of it in the above equation, is a periodic quantity of the third order.

Hence it follows that the quantity

$$-C - \frac{h^2}{r^2} + \frac{2\mu}{r} + \frac{m'r^2}{2a'^3}$$

may vanish for values of r different from those which make $\left(\frac{dr}{dt}\right)^2$ vanish, and that it may even become negative for actual values of r, which $\left(\frac{dr}{dt}\right)^2$ itself can never do.

Therefore the coefficient of dr in the above differential equation may become infinite, or even imaginary, within the limits of in-

tegration, so that it is not surprising that Professor Challis should have met with such difficulties in performing the integration.

The relations between r, θ, and t, given in page 281 (which profess to include all small quantities of the second order), are said to be derived from the equations (B) and (C). It is easy to see, however, that they do not satisfy the first of those equations, since the term of the second order

$$\frac{3m'\rho^2}{2d'^3} \cos 2\overline{\theta - \theta'}$$

in the right-hand member of that equation involves the *longitude of the sun*, which does not occur at all in the relations in question.

The contradiction to Professor Challis's theory, which is presented by the eccentricity of the orbit of Titan, is supposed by him to be occasioned by the large inclination of that orbit to the plane of the orbit of Saturn. But in page 280 it is remarked that the inclination of the orbit is taken into account; and even if this were not the case, no proof is offered that the taking it into account would tend to reconcile the discrepancy.

At the bottom of page 282, Professor Challis attempts to show, *à priori,* that the eccentricity of the moon's orbit must be a function of the disturbing force in the following manner.

If there were no disturbing force, the value of the radius vector drawn from the earth's centre in a given direction, would be constantly the same in different revolutions. But if a disturbing force act in such a manner as to cause the apsidal line to make complete revolutions, the value of the above-mentioned radius-vector would fluctuate in different revolutions, between the two apsidal distances. Hence it is argued that, since if there were no disturbing force there would be no such fluctuation of distance, therefore the total amount of such fluctuation, and consequently the eccentricity, must be a function of the disturbing force.

But, on consideration, it will appear that this argument is fallacious. No doubt it may be inferred that *some of the circumstances* of this fluctuation of distance will depend on the disturbing force which causes it, but it cannot be asserted, without investigation, that the *total amount* of such fluctuation must necessarily depend on the disturbing force.

As a simple example, we will suppose the principal force to vary inversely as the square of the distance, and a central disturbing force to be introduced which varies inversely as the cube of that distance. In this case we know, by Newton's 9th section, that the motion would be accurately represented by supposing it to take place in a revolving ellipse, the angular

velocity of the orbit being always proportional to that of the body at the same instant; and the eccentricity of the orbit might be any whatever, and would not at all depend on the disturbing force.

Now, since the orbit would be fixed, were it not for the disturbing force, it might be argued in exactly the same manner as is done by Professor Challis in the passage above referred to, that the eccentricity of the orbit must be a function of the force which causes the orbit to revolve, but this we know to be a false conclusion.

What would depend on the disturbing force in this case, would be, not the *total amount* of the fluctuation of distance in different revolutions, but the *number of revolutions of the body in which such fluctuation would take place,* or *the time of revolution of the apse.* If the disturbing force were increased, the total fluctuation in the value of the radius-vector in question would be the same as before, but the change from one of the extreme values to the other would occupy a shorter time.

The objection mentioned by Professor Challis at the top of page 283, is alone quite fatal to the supposition that the eccentricity of the moon's orbit must have a particular value. Where is the proof that the eccentricity would *settle down* to such a value, as Professor Challis imagines, if it were initially different?

In fact, it is easy to show, by the method of variation of elements, that there would be no such settlement, but that the non-periodic part of the eccentricity would remain constant.

<div style="text-align:center">I have the honour to be, Gentlemen,
Your obedient Servant,
J. C. Adams.</div>

Pembroke College, Cambridge,
   June 20, 1854.

<div style="text-align:center">

VI. *On the Action of Bromine on Nitropicric Acid.*
   By John Stenhouse, *LL.D., F.R.S.*[*]

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IT is stated, on the authority of Marchand, at p. 683 in the 5th volume of Gmelin's Handbook of Chemistry, that bromine has no action on nitropicric acid. As this assertion appeared to me highly improbable on several grounds, I was induced to undertake the following series of experiments. The nitropicric acid on which I operated was made by treating the resin of the *Xanthorrhœa hastilis* (the yellow gum-resin of Australia) with nitric acid. The resin, which costs only from fourpence to sixpence per pound, yields nearly half its weight of

nitropicric acid, and is therefore by far its most œconomical source.

A quantity of nitropicric acid was digested with water and bromine in a retort, so connected with a condensing apparatus that the bromine and acid vapours, on cooling, flowed back into the mixture. As cork was rapidly attacked and destroyed by the bromine vapours, the different parts of the apparatus were connected by means of plaster of Paris. The mixture of bromine and nitropicric acid was digested for several hours; and when the greater portion of the bromine had disappeared, new quantities of it were added from time to time, and the digestion continued. Bromopicrine and bromanil were, with the exception of hydrobromic acid, almost the sole products. Permanent gases were evolved only in comparatively small quantity, their amount increasing towards the close of the digestion. No carbonic acid was evolved, and the gases consisted chiefly of nitrogen and its oxides, especially the binoxide. The production of these gases appears to arise from the decomposition of bromopicrine. It seems, therefore, that nitropicric acid, when it is digested with bromine, is resolved into bromanil and bromopicrine, probably as follows:—

$$2\big(C^{12}H^3(NO^4)^3O^2\big)+28Br=C^{12}Br^4O^4+6\big(C^2Br^3(NO^4)\big)+6HBr.$$
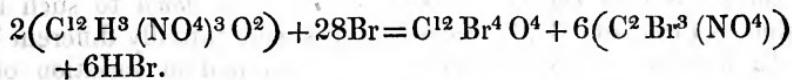
Bromopicrine, $C^2Br^3NO^4$.—When the products obtained by the preceding operation are rectified, bromopicrine of a deep yellow colour, from containing excess of bromine, distils over, and forms an oily fluid under the aqueous solution which has come over with it. It was washed with water containing a little carbonate of soda, and then agitated with mercury to remove any adhering bromine. Although the boiling-point of bromopicrine is much higher than 212° F., it came over chiefly with the first portions of the water, and was perfectly colourless when the excess of bromine had been previously removed. But when bromopicrine had been rendered anhydrous by contact with fused chloride of calcium, though it might be heated to nearly its boiling-point without alteration, yet when it began to boil it was partially decomposed, with the evolution of brownish-red vapours, even though the operation was conducted in an atmosphere of carbonic acid gas. As it was plain therefore that anhydrous bromopicrine could not be purified by distillation, it was separated from the chloride of calcium by filtration, none of the chloride of calcium being retained in solution. The bromopicrine prepared in the way just described, when subjected to analysis, gave 85·1, 85·2, and 85·6 per cent. of bromine. The formula of bromopicrine ($C^2Br^3NO^4$) requires only 80·54 per cent.

of bromine. It is clear from the results of the above analyses, therefore, that the bromopicrine prepared in the manner just described is impure, and contains a quantity of a substance which is richer in bromine, most probably the carburet of bromine (C^2Br^4), which may be readily produced by the long-continued action of bromine on bromopicrine. It is necessary, therefore, to prepare bromopicrine by some other process where such a decomposition is not likely to be produced. Nitropicric acid was consequently digested with excess of hypobromite of lime, in a manner precisely similar to that by which I many years ago prepared chloropicrine. The bromopicrine obtained was washed with solution of carbonate of soda, agitated with mercury, and rendered anhydrous by chloride of calcium. When analysed—,

I. 0·6645 grm., when ignited with quick lime, gave 1·2495 grm. of bromide of silver.

II. 0·5225 grm., when ignited with quick lime, gave 0·9825 grm. of bromide of silver.

		Theory.	Found.	
			I.	II.
C^2	$= 12$	4·03		
Br^4	$= 240$	80·54	80·01	80·02
NO^4	$= 46$	15·43		
	298	100·00		

Bromopicrine is a colourless liquid, which is heavier than water; its odour very closely resembles that of chloropicrine, and its vapour attacks the eyes very strongly. It is very slightly soluble in water, but readily dissolves in alcohol and æther. Its alcoholic solution is not immediately precipitated by nitrate of silver; but on standing for some time, even in the cold, and immediately on the application of heat, bromide of silver precipitates. When bromopicrine is strongly heated, it is decomposed with a slight explosion. Bromopicrine is therefore a much less stable compound than chloropicrine, which in most other respects it so closely resembles.

Bromanil, $C^{12}Br^4O^4$.—The residual matters remaining in the retort after the distillation of the bromopicrine produced by the digestion of bromine with nitropicric acid, were found to consist chiefly of bromanil and some undecomposed nitropicric acid. The excess of nitropicric acid was removed by repeated digestion with hot water, when a quantity of impure bromanil remained as yellowish-red crystalline scales. These scales contained a small quantity of a reddish resinous substance, which was readily removed from the bromanil by taking advantage of its greater solubility in alcohol and æther. When the bromanil had been twice crystallized out of alcohol, it formed beautiful crystal-

line scales of a golden lustre, very closely resembling chloranile or iodide of lead. When heated, it melted, forming a brownish liquid, and readily sublimed, yielding sulphur-yellow crystals. It is nearly insoluble in water, slightly soluble in cold, but tolerably soluble in hot alcohol and in æther. Bromanil crystallized out of alcohol, when analysed, gave the following results :—

0·3695 grm., burnt with chromate of lead, gave 0·2325 grm. of carbonic acid.

[The small quantity of water found was equal to 0·19 per cent. of hydrogen.]

0·3925 grm., burnt with caustic lime, gave 0·6985 grm. of bromide of silver.

		Theory.	Found.
$C^{12} =$	72	16·98	17·16
$Br^4 =$	320	75·47	75·73
$O^4 =$	32	7·55	7·11
	424	100·00	100·00

Bromhydranil, $C^{12} Br^4 H^2 O^4$.—When sulphurous acid gas is passed through hot spirits of wine containing an excess of bromanil, the bromanil gradually dissolves, and a colourless solution is produced. When this solution is sufficiently concentrated, bromhydranil is deposited in colourless crystals, which have a mother-of-pearl lustre. When bromanil is boiled with an aqueous solution of sulphurous acid, bromhydranil is also produced, but extremely slowly, because bromanil and bromhydranil are both nearly insoluble in water.

Bromhydranil is very soluble in alcohol and in æther, but it is nearly insoluble in water, and is therefore precipitated as a white crystalline powder from its concentrated alcoholic solutions on the addition of water. When heated, it melts and readily sublimes, forming soft colourless scales.

0·9412 grm. of bromhydranil, crystallized out of spirits of wine, when burned with chromate of lead, gave 0·5915 grm. of carbonic acid, and 0·0485 grm. of water.

		Theory.	Found.
$C^{12} =$	72	16·90	17·14
$Br^4 =$	320	75·12	
$H^2 =$	2	0·47	0·55
$O^4 =$	32	7·51	
	426	100·00	

Bromanilic Acid, $C^{12} Br^2 H^2 O^8$.—When bromanil is thrown into hot potash lye, it immediately dissolves, forming a deep

purple solution, out of which dark brownish-red needles of a potash salt are speedily deposited, as they are insoluble in the alkaline liquid.   This potash salt readily dissolves in water, but it is nearly insoluble in alcohol.   Its crystals, after being washed with alcohol, were dried *in vacuo* over sulphuric acid, and subjected to analysis.

0·325   grm. gave 0·143   grm. of sulphate of potash.
0·2575 grm. gave 0·2475 grm. of bromide of silver.

According to these determinations, therefore, this salt contains (like the corresponding chloranilate of potash analysed by Erdmann) 2 equivs. of water, as is shown by the following results :—

		Theory.	Found.
$C^{12} =$	72	18·35	
$Br^2 =$	160	40·77	40·87
$H^2 =$	2	0·51	
$K^2 =$	78·4	19·98	19·78
$O^{10} =$	80	20·39	
	392·4	100·00	

The aqueous solution of bromanilate of potash very closely resembles, in its reactions on metallic solutions, those of the chloranilate of potash.   It forms with solutions of the greater number of the heavy metals and with that of baryta, difficultly soluble precipitates, whose colours very much resemble those produced by the chloranilate of potash.   If sulphuric or hydrochloric acids are added to an aqueous solution of bromanilate of potash, or to a solution of bromanil in hot potash lye, the purple colour of the solution immediately disappears, and bromanilic acid is gradually deposited in beautiful, shining, crystalline scales of a reddish colour, which when dried are of a bronze colour.

Bromanilic acid is not precipitated by acetic acid.

0·2735 grm. of the crystals, when ignited with caustic lime, gave 0·3465 grm. of bromide of silver.

		Theory.	Found.
$C^{12} =$	72	24·16	
$Br^2 =$	160	53·69	53·91
$H^2 =$	2	0·69	
$O^3 =$	64	21·46	
	298	100·00	

The solutions of bromanilic acid in alcohol and water are of a deep purple colour.   Its solution in æther is yellowish, but

becomes purple on the addition of alcohol. Long-continued digestion of bromanil with water appears slowly to produce a small quantity of bromanilic acid; at any rate, water, when long boiled with bromanil, gradually becomes of a purple colour.

Bromanilamide ($C^{12} N^2 Br^2 H^4 O^4$) is prepared by passing dry ammoniacal gas into a hot solution of bromanil in alcohol containing some bromanil in suspension. Bromanilamide is also prepared when a mixture of bromanil and alcohol is heated along with concentrated liquor ammoniæ. A smaller quantity of bromanilamide is obtained by this process than when all the substances employed are anhydrous, as a larger amount of the bromanilamate of ammonia, which dissolves in the spirit, is produced.

Bromanilamide forms a brownish red crystalline powder, which is nearly insoluble in water, alcohol and æther. It sublimes, under partial decomposition, in brown-coloured crystals. That portion of the bromanilamide subjected to analysis had been previously dissolved in a weak alcoholic solution of potash, and precipitated by acetic acid.

0·2555 grm. gave 0·3255 grm. of bromide of silver.

		Theory.	Found.
$C^{12} =$	72	24·32	
$N^2 =$	28	9·46	
$Br^2 =$	160	54·06	54·21
$H^4 =$	4	1·35	
$O^4 =$	32	10·81	
	296	100·00	

Bromanilamic Acid.—Bromanil, in its reactions with aqueous ammonia, appears closely to resemble chloranil. The brownish-red solution of bromanil in strong aqueous ammonia, deposited deep brownish-red needles of a salt which appears to be bromanilamate of ammonia. When sulphuric acid is cautiously added to an aqueous solution of this salt, bromanilamic acid precipitates in nearly black-coloured needles. If, while neutralizing the solution of this salt, any considerable elevation of temperature is not carefully avoided, the solution becomes colourless, and crystals are deposited which appear to be bromanilic acid.

St. Bartholomew's Hospital,
June 22, 1854.

VII. *On certain recent Investigations in the Theory of Light.*
By Professor STOKES.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the recently published Part of the Philosophical Transactions occurs a paper by my friend the Rev. Joseph Power, entitled "Theory of the Reciprocal Action between the Solar Rays and the different Media by which they are reflected, refracted, or absorbed; in the course of which various optical laws and phænomena are elucidated and explained," for a copy of which I am indebted to the kindness of the author. The results arrived at in this paper are of a remarkably novel and therefore attractive character, but the investigations appear to me to lie open to the gravest objections. I am at present engaged in discussing the matter privately with Mr. Power, and I am unwilling to trouble the public and fill your pages with the details of the controversy. If, as I hope and expect, Mr. Power and I come ultimately to agree, our views can be laid before the public with much more brevity. My only object in mentioning the subject at present is to show that the question has been taken up.

I am, Gentlemen,

Your faithful Servant,

G. G. STOKES.

Pembroke College, Cambridge,
June 20, 1854.

VIII. *On the Mathematical Theory of Electricity in Equilibrium.*
By WILLIAM THOMSON, *B.A., Fellow of St. Peter's College.*

[Extracted from the "Cambridge and Dublin Mathematical Journal," Nov. 1845. Notes and additions of the present date, March 1854, are enclosed in brackets.]

I. *On the Elementary Laws of Statical Electricity*.*

1. THE elementary laws which regulate the distribution of electricity on conducting bodies have been determined by means of direct experiments, by Coulomb, and in the form he has given them, which is independent of any hypothesis†, they have long been considered as rigorously established. The problem of the distribution of electricity in equilibrium on a conductor of any form was thus brought within the province of mathematical analysis; but the solution, even in the simplest cases, presented so much difficulty that Coulomb, after having

* This paper is a translation by the author (with considerable additions) of one which appeared in Liouville's *Journal de Mathématique*, vol. **x.** p. 209.

† See the first Note at the end of this paper.

investigated it experimentally for bodies of various forms, could only compare his measurements with the results of his theory by very rude processes of approximation. Without however giving rigorous solutions in particular cases, he examined the general problem with great care, and left nothing indefinite in the conditions to be satisfied, so that it was entirely by analytical difficulties that he was stopped. As an example of the success of his theoretical investigations, we may refer to the well-known demonstration of the theorem (usually attributed to Laplace) relative to the repulsion exercised by a charged conductor on a point near its surface[*].

The memoirs of Poisson, on the mathematical theory, contain the analytical determination of the distribution of electricity on two conducting spheres placed near one another, the solution being worked out in numbers in the case of two equal spheres in contact, which had been investigated experimentally by Coulomb (as well as in another case, not examined by Coulomb, which is given as a specimen of the numerical results that may be deduced from the formulæ). The calculated ratios of the intensities at different points of the surface he is therefore enabled to compare with Coulomb's measurements, and he finds an agreement which is quite as close as could be expected, when we consider the excessively difficult and precarious nature of quantitative experiments in electricity. But the most remarkable confirmation of the theory from these researches is the entire agreement of the principal features, even in some very singular phænomena, of the experimental results with the theoretical deductions. For a complete account of the experiments, we must refer to Coulomb's fifth memoir (*Histoire de l'Académie,* 1787), and for the mathematical investigations to the first and second memoirs of Poisson (*Mémoires de l'Institut,* 1811), or to the treatise on Electricity in the "Encyclopædia Metropolitana," where the substance of Poisson's first memoir is given.

The mathematical theory received by far the most complete development which it has hitherto obtained in Green's "Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism[†]," in which a series of general theorems were demonstrated, and many interesting applications made to actual problems[‡].

Of late years some distinguished experimentalists have begun to doubt the truth of the laws established by Coulomb, and have made extensive researches with a view to discover the laws of certain phænomena which they considered incompatible with his theory. The most remarkable works of this kind have been

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[*] See Note II.   [†] Nottingham, 1828.   [‡] See Note III.

undertaken by Mr. Snow Harris and Mr. Faraday, and in their memoirs, published in the Philosophical Transactions, we find detailed accounts of their researches. All the experiments, however, which they have made, having direct reference to the distribution of electricity in equilibrium, are, I think, in full accordance with the laws of Coulomb, and must therefore, instead of objections to his theory, be considered as confirming it. As however many have believed Coulomb's theory to be overturned by these investigations, and as others have at least been led to entertain doubts as to its certainty or accuracy, the following attempt to explain the apparent difficulties is made the subject of the first of a series of papers in which various parts of the mathematical theory of electricity, and corresponding problems in the theories of magnetism and heat, will be considered.

2. We may commence by examining some experimental results published in Mr. Harris's first memoir "On the Elementary Laws of Electricity[*]." After describing the instruments employed in his researches, Mr. Harris gives the details of some experiments with reference to the attraction exercised by an insulated electrified body on an uninsulated conductor placed in its neighbourhood. The first result which he announces is that, when other circumstances remain the same, the attraction varies as the square of the quantity of electricity with which the insulated body is charged. It is readily seen, as was first remarked by Dr. Whewell in his "Report on the Theories of Electricity, &c.[†]," that this is a rigorous deduction from the mathematical theory, following from the fact that the quantity of electricity induced upon the uninsulated body is proportional to the charge on the electrified body by which it is attracted.

The remaining results have reference to the force of attraction at different distances, and with bodies of different forms opposed. As these are generally very irregular (such as "plane circular areas backed by small cones"), we should not, according to Coulomb's theory, expect any very simple laws, such as Mr. Harris discovers, to be rigorously true. Accordingly, though they are announced by him without restriction, we must examine whether the experiments from which they have been deduced are of a sufficiently comprehensive character to lead to any general conclusions with respect to electrical action. Now in the first place, we find that in all of them the attraction is "independent of the form of the unopposed parts" of the bodies, which will be the case only when the intensity of the induced electricity on the unopposed parts of the uninsulated

* Philosophical Transactions, 1834.
† British Association Report for 1837.

body is insensible. According to the mathematical theory, and according to Mr. Faraday's researches "on induction in curved lines," which will be referred to below, the intensity never absolutely vanishes at any point of the uninsulated body: but it is readily seen that in the case of Mr. Harris's experiments, it will be so slight on the unopposed portions that it could not be perceived without experiments of a very refined nature, such as might be made by the proof plane of Coulomb, which is in fact, with a slight modification, the instrument employed by Mr. Faraday in the investigation. Now to the degree of approximation to which the intensity on the unopposed parts may be neglected, the laws observed by Mr. Harris when the opposed surfaces are plane may be readily deduced from the mathematical theory. Thus let v be the potential in the interior of the charged body, A a quantity which will depend solely on the state of the interior coating of the battery with which in Mr. Harris's experiments A is connected, and will therefore be sensibly constant for different positions of A relative to the uninsulated opposed body B. Let a be the distance between the plane opposed faces of A and B, and let S be the area of the opposed parts of these faces, which will in general be the area of the smaller, if they be unequal. When the distance a is so small that we may entirely neglect the intensity on all the unopposed parts of the bodies, it is readily shown from the mathematical theory that (since the difference of the potentials at the surfaces of A and B is v) the intensity of the electricity produced by induction at any point of the portion of the surface of B which is opposed to A, is $\dfrac{v}{4\pi a}$, the intensity at any point which is not so situated being insensible. Hence the attraction on any small element ω, of the portion S of the surface of B, will be in a direction perpendicular to the plane and equal to $2\pi \left(\dfrac{v}{4\pi a}\right)^2$ *. Hence the whole attraction on B is

$$\frac{v^2 S}{8\pi a^2}.$$

This formula expresses all the laws stated by Mr. Harris as results of his experiments in the case when the opposed surfaces are plane.

3. When the opposed surfaces are curved, for instance, when A and B are equal spheres, we can make no approximation analogous to that which has led us to so simple an expression in the case of opposed planes; and we find accordingly that no such simple law for the attraction in this case has been

* See Mathematical Journal, vol. iii. p. 275.

announced by Mr. Harris. He has, however, found that it is expressed with tolerable accuracy by the formula

$$F = \frac{k}{c(c-2a)},$$

where c is the distance between the centres of the spheres, a the radius of each, k a constant, which will depend on a and on the charge of the battery with which A is in communication. Though however this formula may give results which do not differ very much from observation within a limited range of distances, it cannot, according to any theory, be considered as expressing the physical law of the phænomenon. For, according to it, when the balls are very distant, F ultimately varies as $\frac{1}{c^2}$.

Now it is clear that the law of force must ultimately become the inverse cube of the distance, since the quantity of electricity induced upon B will be ultimately in the inverse ratio of the distance, and the attraction between the balls as the product of the quantities of electricity directly, and as the square of the distance inversely, and hence the formula given by Mr. Harris cannot express the law of force when the balls are very distant. In the experiments by which his formula is tested, the force of attraction is measured by means of an ordinary balance and weights : the only comparison of results which he publishes is transcribed in the following table.

Distance of centres.	Measured force in grains.	Values of $\frac{15c_1(c_1-2)}{c(c-2)}$.
$c_1 = 2 \cdot 3$	15	15
$c_2 = 2 \cdot 5$	$8 \cdot 25 +$	$8 \cdot 28$
$c_3 = 2 \cdot 8$	$4 \cdot 6 \ +$	$4 \cdot 62$
$c_4 = 3 \cdot 0$	$3 \cdot 5 \ -$	$3 \cdot 45$

From this table we see that the formula is verified in three cases to the extent of accuracy of the experiments. Comparisons extended to a much wider range of distances would be required to establish it, and it would be necessary to take precautions to prevent the experimental results from being influenced by disturbing causes. In the experiments made by Mr. Harris we find that no precautions have been taken to avoid the disturbing influence of extraneous conductors, which, according to the descriptions and drawings he gives of his instruments, seem to exist very abundantly in the neighbourhood of the bodies operated upon, being partly metal in connexion with the insulated system with which the body A communicates, and partly uninsulated metal, in the fixed parts of the electrometer, and in the moveable parts by which B is supported. The general effect

produced by the presence of such bodies in disturbing the observed law of force, must be to make it diminish less rapidly with the distance when A and B are separated by a considerable interval: and it is probably owing, at least in part, to such disturbing causes that Mr. Harris's results nearly agree, as far as they go, with a formula which would ultimately give for the law of force the inverse square of the distance between A and B, instead of the inverse cube.

4. The determination by the mathematical theory of the attraction or repulsion between two electrified conducting spheres has not hitherto, so far as I am aware, been attempted, and would present considerable difficulty by means of the formulæ ordinarily given for such problems. It may, however, very readily be effected by means of a general theorem on the attraction between electrified conductors, which will be given in a subsequent paper *. Thus, if F (c) be the force of attraction, corresponding to the distance c between the centres, in the particular case when the two spheres are equal (the radius of each being unity), and the potential in the interior of one of them is nothing (as will be the case when the body is uninsulated), the potential in the interior of the other being v, I have found the following formulæ which express F (c) by a converging series.

$$F(c) = v^2 c \left(\frac{P_1}{Q_1^2} + \frac{P_2}{Q_2^2} + \frac{P_3}{Q_3^2} + \&c. \right), \quad . \quad . \quad (A)$$

where

$$\left. \begin{array}{l} Q_1 = c^2 - 1, \\ Q_2 = (c^2 - 2) Q_1 - 1, \\ Q_{n+2} = (c^2 - 2) Q_{n+1} - Q_n. \end{array} \right\} \quad . \quad . \quad . \quad (B)$$

$$\left. \begin{array}{l} P_1 = 1, \\ P_2 = 2c^2 - 3, \\ P_{n+2} = (c^2 - 2) P_{n+1} + (Q_{n+1} - P_n). \end{array} \right\} \quad . \quad . \quad (C)$$

These formulæ enable us to calculate Q_1, Q_2, Q_3, Q_4, &c., and then P_1, P_2, P_3, P_4, &c., successively, by a simple and uniform arithmetical process, for any particular value of c. I have thus calculated the values of $\frac{F(c)}{v^2}$ in five cases, the first four of which are those examined by Mr. Harris, and have obtained

[* The enunciation of the "general theorem" alluded to, the investigation founded on it, by which the author first arrived at the conclusion made use of here, and another demonstration of the same conclusion, founded on the method of electrical images, and strictly synthetical in its character, are published, with comprehensive numerical results, in the Philosophical Magazine for April 1853.]

the following results, each of which is true to five places of decimals.

c.	$v^{-2}F(c)$.
2·3	0·32926
2·5	0·17423
2·8	0·09168
3·0	0·06592
4·0	0·02075

To compare these with Mr. Harris's measurements we may calculate the value of the potential in his battery, during the observations, by means of his first result, and thence find the attraction for the other three cases by means of the calculated values of $v^{-2}F$ (c). Thus we have $v^{-2} \times 15 = 3293$, which gives

$$v^2 = 45\cdot56,$$

and hence

$$F(2\cdot5) = 7\cdot94,$$
$$F(2\cdot8) = 4\cdot18,$$
$$F(3) = 3\cdot00.$$

These numbers differ considerably from Mr. Harris's results, but in the direction indicated by the considerations mentioned above.

5. The most important part of the researches of Mr. Harris is that in which he investigates the insulating power of air of different densities. The result at which he arrives is, that the intensity necessary to produce a spark depends solely on the density of the air, and not otherwise on the pressure or temperature. He thus shows that the conducting power of flame, of heated bodies, and of a vacuum, are due solely to the rarefaction of the air in each case. He also shows that the intensities necessary to produce a spark, are in the simple ratios of the densities of the air.

6. In a subsequent memoir, by the same author[*], we find additional experiments on the elementary principles of the theory of electricity. The first series which is described, was made for the purpose of testing the truth of Coulomb's law, that the repulsion of two similarly charged points is inversely as the square of the distance, and directly as the product of the masses. In experiments of this kind in which accurate quantitative results are aimed at, many precautions are necessary. Thus all conducting bodies except those operated upon, must be placed beyond the reach of influence, and the distance between the repelling bodies must be considerable with reference to their

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[*] Philosophical Transactions, 1836.

linear dimensions, so that the distribution of electricity on each may be uninfluenced by the presence of the other. Also the bodies should be spheres, so that the attraction may be the same as if the whole electricity of each were collected at its centre; and the distance to be measured will then be the distance between the centres. These conditions have been expressly mentioned by Coulomb, and they have been fulfilled, as far as possible, in his researches, as we see by the descriptions of the experiments made, which we find in his memoirs. He has thus arrived by direct measurement at the law, which we know by a mathematical demonstration*, founded upon independent experiments, to be the rigorous law of nature, for electrical action. None of these precautions however have been taken in the experiments described in Mr. Harris's memoir, and the results are accordingly unavailable for the accurate *quantitative* verification of any law, on account of the numerous unknown disturbing circumstances by which they are affected. The phænomena which he observes, however, afford *qualitative* illustrations of the mathematical theory of a very interesting nature, as may be seen from the following examples of his results.

(*a*) When the distance between the bodies is great with reference to their linear dimensions, the repulsion is inversely as the square of the distance, and directly as the product of the masses.

(*b*) When the distance is small, the action becomes apparently irregular. Thus if the quantities of electricity on the two bodies be equal, the force, which is always of repulsion, does not increase so rapidly when the bodies approach, as if it followed the law of the inverse square of the distance.

(*c*) If the charges be unequal, the repulsion ceases at a certain

* See Murphy's "Electricity," p. 41, or Pratt's "Mechanics," Art. 154.

[Cavendish demonstrates mathematically that if the law of force be any other than the inverse square of the distance, electricity could not rest in equilibrium on the surface of a conductor. But experiment has shown, that electricity does rest at the surface of a conductor. Hence the law of force must be the inverse square of the distance. Cavendish considered the second proposition as highly probable, but had not experimental evidence to support this opinion, in his published work (An attempt to explain the phænomena of Electricity by means of an Elastic Fluid). Since his time, the most perfect experimental evidence has been obtained that electricity resides at the surface of a conductor; in such facts, for instance, as the perfect equivalence in all electro-statical relations of a hollow metallic conductor of ever so thin substance, or of a gilt non-conductor (possessing a conducting film of not more than $\frac{1}{300000}$ of an inch thick) and a solid conductor of the same external form and dimensions; the minor premise of his syllogism is thus demonstrated, and the conclusion is therefore established.]

distance, and at all smaller distances there is attraction between the bodies.

These results are, with all their peculiarities, in full accordance with the theory of Coulomb, which indicates that, if the quantities of electricity be equal, and the bodies equal and similar, there will be repulsion in every position : but if there be any difference, however small, between the charges, the repulsion will necessarily cease, and attraction commence, before contact takes place, when one body is made to approach the other. Unless, however, the difference of the charges be sufficiently considerable, a spark may pass between the bodies, and render the charges equal, before attraction commences. In Mr. Harris's experiments, in which the bodies seem to have been nearly oblate spheroids, the attraction is generally sensible before the distance is small enough to allow a spark to pass, if the charge on one be double of that on the other.

Mr. Harris next proceeds to investigate the theory of the proof plane, and to examine whether it can be considered as indicating with certainty the intensity of electricity at any part of a charged body, and, principally from an experiment made on a charged non-conductor (a hollow sphere of glass), comes to a negative conclusion. It should be remembered, however, that, the proof plane having never been applied to determine the intensity at points of the surface of a charged non-conductor, such conclusions in no way interfere with adopted ideas. Since there can be no manner of doubt as to the theory of this valuable instrument, as we find it explained by M. Pouillet*, nor as to the experimental use of it made by Coulomb, it is unnecessary to enter more at length on the subject here.

7. Mr. Faraday's researches on electro-statical induction, which are published in a memoir forming the eleventh series of his "Experimental Researches in Electricity," were undertaken with a view to test an idea which he had long possessed, that the forces of attraction and repulsion exercised by free electricity are not the resultant of actions exercised at a distance, but are propagated by means of molecular action among the contiguous particles of the insulating medium surrounding the electrified bodies, which he therefore calls the *dielectric*. By this idea he has been led to some very remarkable views upon induction, or in fact upon electrical action in general. As it is impossible that the phænomena observed by Faraday can be incompatible with the results of experiment which constitute Coulomb's theory, it is to be expected that the difference of his ideas from those of Coulomb must arise solely from a different method of stating, and interpreting physically, the same laws :

* See Note IV.

and further, it may I think be shown that either method of viewing the subject, when carried sufficiently far, may be made the foundation of a mathematical theory which would lead to the elementary principles of the other as consequences. This theory would accordingly be the expression of the ultimate law of the phænomena, independently of any physical hypothesis we might, from other circumstances, be led to adopt. That there are necessarily two distinct elementary ways of viewing the theory of electricity, may be seen from the following considerations, founded on the principles developed in a previous paper in this Journal*.

Corresponding to every problem relative to the distribution of electricity on conductors, or to forces of attraction and repulsion exercised by electrified bodies, there is a problem in the uniform motion of heat which presents the same analytical conditions, and which, therefore, considered mathematically, is the same problem. Thus, let a conductor A, charged with a given quantity of electricity, be insulated in a hollow conducting shell, B, which we may suppose to be uninsulated. According to the mathematical theory, an equal quantity of electricity of the contrary kind will be attracted to the interior surface of B (or the surface of B, as we may call it to avoid circumlocution), and the distribution of this charge, and of the charge on A, will take place so that the resultant attraction at any point of each surface may be in the direction of the normal. This condition being satisfied, it will follow that there is no attraction on any point within A, or without the surface of B, that is, on any point within either of the conducting bodies. The most convenient mathematical expression for the condition of equilibrium, is that the potential at any point P† must have a constant value when P is on the surface of A, and the value nothing when P is on the surface of B; and it will follow from this that the potential will have the same constant value for any point within A, and will be equal to nothing for any point without the surface of B.

If A be subject to the influence of any uninsulated conductors, we must consider such bodies as belonging to the shell in which A is contained, and their surfaces as forming part of the surface of B: in such cases this surface will generally be the interior surface of the walls of the room in which A is contained, and of all uninsulated conductors in the room. If, however, we

* On the Uniform Motion of Heat, and its Connexion with the Mathematical Theory of Electricity, vol. iii. p. 73 [Phil. Mag. S. 4. vol. vii. p. 502].

† The term used by Green for the sum of the quotients obtained by dividing the product of each element of the surfaces of A and B, and its electrical intensity, by its distance from P.

have to consider the case in which A is subject to no external influence, we must suppose every part of the surface of B to be very far from A.   The most general problem we can contemplate in electricity (exclusively of the case in which the insulating medium is heterogeneous, and exercises a special action, which will be alluded to below), is to determine the potential at any point when A, instead of being a single conductor, is a group of separate insulated conductors charged to different degrees, and when there are non-conductors electrified in a given manner, placed in the insulating medium, in the neighbourhood.   The conditions of equilibrium will still be that the potential at each surface due to all the free electricity must be constant, and the theorems stated above will still be true : thus the attraction will be nothing in the interior of each portion of A, and without the surface of B ; and the whole quantity of induced electricity on the latter surface will be the algebraic sum of the charges of all the interior bodies with its sign changed.   When the potential due to such a system is determined for every point, the component of the resultant force at any point P, in any direction PL, may be found by differentiation, being the limit of the difference between the values of the potential at P, and at a point Q, in PL, divided by PQ, when P moves up towards and ultimately coincides with P, and the direction of the force, on a *negative* particle, being that in which the potential increases.   By Coulomb's theorem, the intensity at any point in one of the conducting surfaces is equal to the attraction (on a negative unit), at that point, divided by 4π.

Now if we wish to consider the corresponding problem in the theory of heat, we must suppose the space between A and B, instead of being filled with a dielectric medium (that is, a non-conductor for electricity), to be occupied by any homogeneous solid body, and sources of heat or cold to be so distributed over the terminating surfaces, or the interior surface of B and the surface of A, that the permanent temperature at the first surface may be zero, and at the second shall have a certain constant value, the same as that of the *potential* in the case of electricity. If A consist of different isolated portions, the temperature at the surface of each will have a constant value, which is not necessarily the same for the different portions.   The problem of *distributing sources of heat, according to these conditions,* is mathematically identical with the problem of distributing *electricity in equilibrium* on the surfaces of A and B.   In the case of heat, the *permanent temperature* at any point replaces the *potential* at the corresponding point in the electrical system, and consequently the *resultant flux of heat* replaces the *resultant attraction* of the electrified bodies, in direction and magnitude.

The problem in each case is determinate, and we may therefore employ the elementary principles of one theory, as theorems, relative to the other. Thus, in the paper in which these considerations are developed, Coulomb's fundamental theorem relative to electricity is applied to the theory of heat; and self-evident propositions in the latter theory are made the foundation of Green's theorems in electricity*. Now the laws of motion for heat which Fourier lays down in his *Théorie Analytique de la Chaleur*, are of that simple elementary kind which constitute a mathematical theory properly so called; and therefore, when we find corresponding laws to be true for the phænomena presented by electrified bodies, we may make them the foundation of the mathematical theory of electricity: and this may be done if we consider them merely as actual truths, without adopting any physical hypothesis, although the idea they naturally suggest is that of the propagation of some effect by means of the mutual action of contiguous particles; just as Coulomb, although his laws naturally suggest the idea of material particles attracting or repelling one another at a distance, most carefully avoids making this a *physical hypothesis*, and confines himself to the consideration of the mechanical effects which he observes and their necessary consequences†.

All the views which Faraday has brought forward, and illustrated or demonstrated by experiment, lead to this method of establishing the mathematical theory, and, as far as the analysis is concerned, it would, in most *general* propositions, be even more simple, if possible, than that of Coulomb. (Of course the analysis of *particular* problems would be identical in the two methods.) It is thus that Faraday arrives at a knowledge of some of the most important of the general theorems, which, from their nature, seemed destined never to be perceived except as mathematical truths. Thus, in his theory, the following proposition is an elementary principle. Let any portion *a* of the surface of A be projected on B, by means of lines (which will be in general curved) possessing the property that the resultant electrical force at any point of each of them is in the direction of the tangent: the quantity of electricity produced by induction on this projection is equal to the quantity of the opposite kind of electricity on *a*‡. The lines thus defined are what Faraday calls the "curved lines of inductive action." For

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* It was not until some time after that paper was published, that I was able to add the direct analytical demonstrations of the theorems, which are given in the papers on "General Propositions in the Theory of Attraction," Math. Journ. vol. iii. pp. 189, 201, and which I have since found are the same as those originally given by Green.

† See Note I. ‡ See Note IV.

a detailed account of the experiments by which these phænomena are investigated, reference must be made to Mr. Faraday's own memoirs, published in the Philosophical Transactions, and in a separate form in his "Experimental Researches."

8. The hypothesis adopted by Faraday, of the *propagation* of inductive action, naturally led him to the idea that its effects may be in some degree dependent upon the nature of the insulating medium or dielectric, by which, according to this view, it is transmitted. In the second part of his memoir he describes a series of researches instituted to put this to the test of experiment, and arrives at the following conclusions.

If the dielectric be air, the inductive action is quite independent of its density or temperature (which, as Mr. Faraday remarks, agrees perfectly with previous results obtained by Mr. Harris) ; and in general, if the dielectric be any gas or vapour capable of insulating a charge, the inductive action is invariable. Hence he concludes that "*all gases have the same power of,* or *capacity for,* sustaining induction through them (which might have been expected when it was found that no variation of density or pressure produced any effect.)"

When the dielectric is solid, the induction is greater than through air, and varies according to the nature of the substance. Numbers which measure the "specific inductive capacities" of the dielectrics employed (sulphur, shell-lac, glass, &c.), are deduced from the experiments.

To express these results in the language of the mathematical theory, let us recur to the supposition of a body, A, charged with a given quantity of electricity, and insulated in the interior of a closed conducting shell, B. The potential of the system at the interior surface of B, and at every point without this surface, will be nothing ; at the surface and in the interior of A it will have a constant value, which will depend on the form, magnitude, and relative position of the surfaces A and B, on the quantity of electricity on A, and, according to Faraday's discovery, on the *dielectric power* of the insulating medium which fills the space between A and B. If this be gaseous, neither its nature, nor its state as to temperature, pressure, or density, will affect the value of the potential in A ; but if it be a solid substance, such as sulphur or shell-lac, the value of the potential will be less than when the space is occupied by air, and will vary with the nature of the insulating solid.

The result in the case of a gaseous dielectric is what would follow from Coulomb's theory, if we consider gases to be quite impermeable to electricity, and to be entirely unaffected by electrical influence. The phænomena observed with solid dielectrics, which agree with the circumstance observed by Nicholson,

that the *dissimulating power* of a Leyden phial depends on the nature of the glass of which it is made, as well as on its thickness, have been by some attributed to a slight degree of conducting power, or of penetrability, possessed by solid insulators. This explanation, however, seems to be very insufficient; and besides, Faraday has estimated the nature of the effects of imperfect insulation, by independent experiments, and has established, in what seems to be a very satisfactory manner, the existence of a peculiar action in the interior of solid insulators when subjected to electrical influence. As far as can be gathered from the experiments which have yet been made, it seems probable that a dielectric, subjected to electrical influence, becomes excited in such a manner that every portion of it, however small, possesses *polarity* exactly analogous to the magnetic polarity induced in the substance of a piece of soft iron under the influence of a magnet. By means of a certain hypothesis regarding the nature of magnetic action*, Poisson has investigated the mathematical laws of the distribution of magnetism and of magnetic attractions and repulsions. These laws seem to represent in the most general manner the state of a body polarized by influence, and therefore, without adopting any particular mechanical hypothesis, we may make use of them to form a mathematical theory of electrical influence in dielectrics, the truth of which can only be established by a rigorous comparison of its results with experiment.

Let us therefore consider what would be the effect, according to this theory, which would be produced by the presence of a solid dielectric, C, placed in the space between A and B, the rest of which is occupied by air. The action of C, when excited by the influence of the electricities on A and B, may (as Poisson has shown for magnetism) be represented, whether on points within or without C, by a certain distribution of positive electricity on one portion of the surface of C, and of an equal quantity of negative electricity on the remainder. The condition necessary

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* Faraday adopts the corresponding hypothesis to explain the action of a solid dielectric, which he states thus:—"If the space round a charged globe were filled with a mixture of an insulating dielectric, as oil of turpentine or air, and small globular conductors, as shot, the latter being at a little distance from each other, so as to be insulated, then these in their condition and action exactly resemble what I consider to be the condition and action of the particles of the insulating dielectric itself. If the globe were charged, these little conductors would all be polar; if the globe were discharged, they would all return to their normal state, to be polarized again upon the recharging of the globe."—(Experimental Researches, § 1679.) The results of the mathematical analysis of such an action are given in the text. It may be added that the value of the coefficient k will differ sensibly from unity if the volume occupied by the small conducting balls bear a finite ratio to that occupied by the insulating medium.

and sufficient for determining this distribution may (as can be shown from Poisson's analysis) be expressed as follows. Let R be the resultant force on a point P without C, and R' on a point P' within C, due to the electrified surfaces A and B, and to the imagined distribution on C. If P and P' be taken infinitely near one another, and consequently each infinitely near the surface of C, the component of R' in the direction of the normal must bear to the component of R in the same direction a constant ratio $\left(\dfrac{1}{k}\right)$ depending on the capacity for dielectric induction of the matter of C*. The components of R and R' in the tangent plane will of course be equal and in the same direction, and, if ρ be the intensity of the imagined distribution on the surface of C, in the neighbourhood of P and P', the difference of the normal components will be $4\pi\rho$, as is evident from Coulomb's theorem, referred to above.

Let us now suppose C to be a shell surrounding A, and let S and S', its interior and exterior surfaces, be *surfaces of equilibrium* in the system of forces due to the action of A and B, and of the polarity of C. It may be shown that the same surfaces S, S' would necessarily be surfaces of equilibrium, if C were removed and the whole space were filled with air; and consequently, that the whole series of surfaces of equilibrium, commencing with A and ending with B, will be the same in the two cases. Hence the resultant force due to the excitation of the dielectric C, or to the imagined distributions of electricity on S and S' which produce it, on points within S or without S', must be such as not to alter the distributions on A and B when the quantity on A is given, and is therefore nothing. Accordingly, let Q be the total force on a point indefinitely near S, and within it; Q' the total force on a point without S', but indefinitely near it. Since the forces on points without S and within S' indefi-

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* From this it follows, that, in the case of heat, C must be replaced by a body whose conducting power is k times as great as that of the matter occupying the remainder of the space between A and B.

[The same demonstration, of course, is applicable to the influence of a piece of soft iron, or other " paramagnetic" (*i.e.* substance of ferro-magnetic inductive capacity), or to the reverse influence of a diamagnetic on the magnetic force in any locality near a magnet in which it can be placed, and shows that the lines of magnetic force will be altered by it precisely as the lines of motion of heat in corresponding thermal circumstances would be altered by introducing a body of greater or of less conducting power for heat. Hence we see how strict is the foundation for an analogy on which the *conducting power of a magnetic medium for lines of force* may be spoken of, and we have a perfect explanation of the condensing action of a paramagnetic, and the repulsive effect of a diamagnetic, upon the lines of force of a magnetic field, which have been described by Faraday.—(Exp. Researches, §§ 2807, 2808).]

nitely near the former points are, according to the law stated above, $\dfrac{Q}{k}$ and $\dfrac{Q'}{k}$, it follows that the intensities of the imagined distributions on S and S', in the nieghbourhood of the points considered, are

$$-\frac{1}{4\pi}\left(Q-\frac{Q}{k}\right) \text{ and } \frac{1}{4\pi}\left(Q'-\frac{Q'}{k}\right).$$

Hence, if U, U' be the potentials at S, S', due to A and B alone, and v the potential at any point P, it follows[*] that the potential at P, due to the polarity of the dielectric, is

$$-\left(1-\frac{1}{k}\right)U+\left(1-\frac{1}{k}\right)U',$$

or

$$-\left(1-\frac{1}{k}\right)v+\left(1-\frac{1}{k}\right)U',$$

or

$$-\left(1-\frac{1}{k}\right)v+\left(1-\frac{1}{k}\right)v, \text{ that is } 0,$$

according as P is within S, within S' and without S, or without S'. Hence the total potential will be, according to the position of P,

$$v-\left(1-\frac{1}{k}\right)(U-U'),$$

or

$$\frac{v}{k}+\left(1-\frac{1}{k}\right)U,$$

or

$$v.$$

Hence the sole effect of the dielectric C, on the state of A and B, is to diminish the potential in the interior of the former by the quantity

$$\left(1-\frac{1}{k}\right)(U-U').$$

If the whole space between A and B be occupied by the solid dielectric, the surfaces S and A will coincide, as also S' and B, and therefore U = V, U' = 0. Hence the potential in the interior of A will be

$$\frac{V}{k},$$

or the fraction $\dfrac{1}{k}$ of the potential, with the same charge on A, and with a gaseous dielectric. From this it follows that, when

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[*] See Green's Essay, art. 12; or Math. Journal, vol. iii. p. 75.

the dielectric is solid, it would require, to produce a given potential in the interior of A, k times the charge which would be necessary to produce the same potential when the dielectric is gaseous, and therefore the body A in a given state, defined by the potential in its interior, produces on the interior surface of B, by induction, through the solid dielectric, a quantity of electricity k times as great as through a gaseous dielectric. On this account Faraday calls the property of a dielectric measured by k, its "specific inductive capacity."

In Faraday's experiments an apparatus (which is in fact a Leyden phial, in which any solid or fluid may be substituted for the glass dielectric of an ordinary Leyden phial) is used, corresponding to the case we have been considering, in which A is a conducting sphere (2·33 inches in diameter), and B a concentric spherical shell surrounding it (the distance between the surfaces of A and B being ·62 of an inch). In the shell B there is an aperture into which a shell-lac stem is fixed; a wire, attached to A, passes through the centre of this stem to the outside of the shell, and supports a ball of metal, M, which is thus insulated and connected with A. It may be shown that in such an apparatus the state of the ball A and of the shell B will approximately be not affected by the aperture in the latter, or by the wire supporting M, and that the distribution of electricity on M will be approximately the same as if the wire supporting it and the conductors A and B were removed. Hence the sole relation between A and M will be that the *potentials* in their interiors are the same; and therefore the latter, which is accessible, may be taken as an index of the state of the former.

To determine the specific inductive capacity of any dielectric, Faraday uses two apparatus of the kind just described, precisely equal and similar, in one of which the space between A and B is filled with air, and in the other with the dielectric to be examined. One of these apparatus is charged, and the intensity measured: the balls M, M' in the two are then made to touch and separated again, and the remaining intensity on the first (which is equal to the intensity imparted to the second) is measured. If this be found to differ from half the original intensity, it will follow that the specific inductive capacity of the substance examined differs from that of air, which is unity, and its value may be determined by means of a simple expression from the experimental data. To investigate this, let us first suppose each apparatus to be charged, and let it be required to find the intensity on the balls after they are made to touch, and then removed from mutual influence; and let the dielectrics be any two substances, whose inductive capacities are k, k'. Let ρ, ρ' be the intensities before, and σ the common intensity after

contact. Then, denoting by Q, Q' the quantities of electricity constituting the charges before, and q, q' after contact, we shall have, by the principles already developed,

$$\frac{Q}{Q'} = \frac{k\rho}{k'\rho'}, \quad \frac{\sigma}{\rho} = \frac{q}{Q}, \quad \frac{\sigma}{\rho'} = \frac{q'}{Q'}.$$

Also

$$Q + Q' = q + q'.$$

Hence we deduce

$$\sigma = \frac{k\rho + k'\rho'}{k + k'}.$$

In the experiment described, one of the dielectrics is air. Hence, to obtain the required formula, we may put $k' = 1$, in this equation, and then resolve for k.

Thus we find

$$k = \frac{\sigma - \rho'}{\rho - \sigma}.$$

If only one of the apparatus be originally charged, according as it is the first or the second, we shall have

$$k = \frac{\sigma}{\rho - \sigma},$$

or

$$k = \frac{\rho' - \sigma}{\sigma}.$$

If the substance examined (the dielectric of the first apparatus) be any gas, or air in a different state as to pressure or temperature from the air of the second apparatus, Faraday always finds the intensity after contact to be half the original intensity, and hence for every gaseous body $k = 1$.

If the dielectric of the first apparatus be solid, the intensity after contact is found to be greater than half the original intensity when the first, and less than half when the second is the apparatus originally charged. Hence for a solid dielectric $k > 1$. For sulphur Faraday finds the value to be rather more than 2·2; for shell-lac, about 2; and for flint-glass, greater than 1·76.

The commonly received ideas of attraction and repulsion exercised at a distance, independently of any intervening medium, are quite consistent with all the phænomena of electrical action which have been here adduced. Thus we may consider the particles of air in the neighbourhood of electrified bodies to be entirely uninfluenced, and therefore to produce no effect in the resultant action on any point: but the particles of a solid non-conductor must be considered as assuming a polarized state when under the influence of free electricity, so as to exercise attractions or repulsions on points at a distance, which, with the

action due to the charged surfaces, produce the resultant force at any point. It is no doubt possible that such forces at a distance may be discovered to be produced entirely by the action of contiguous particles of some intervening medium, and we have an analogy for this in the case of heat, where certain effects which follow the same laws are undoubtedly propagated from particle to particle. It might also be found that magnetic forces are propagated by means of a second medium, and the force of gravitation by means of a third. We know nothing, however, of the molecular action by which such effects could be produced, and in the present state of physical science it is necessary to admit the known facts in each theory as the foundation of the ultimate laws of action at a distance.

St. Peter's College, Nov. 22, 1845.

NOTES.

NOTE I.

Coulomb has expressed his theory in such a manner that it can only be attacked in the way of proving his experimental results to be inaccurate. This is shown in the following remarkable passage in his sixth memoir, which follows a short discussion of some of the physical ideas then commonly held with reference to electricity. " *Je préviens pour mettre la théorie qui va suivre à l'abri de toute dispute systématique, que dans la supposition des deux fluides electriques, je n'ai d'autre intention que de présenter avec le moins d'elémens possible, les resultats du calcul et de l'expérience, et non d'indiquer les véritables causes de l'électricité. Je renverrai, à la fin de mon travail sur l'électricité, l'examen des principaux systèmes auxquels les phénomènes électriques ont donné naissance.*"—*Histoire de l'Académie,* 1788, p. 673.

NOTE II.

This theorem may be stated as follows. Let A be a closed surface of any form, and let matter, attracting inversely as the square of the distance, be so distributed over it that the resultant attraction on an interior point is nothing : the resultant attraction on an exterior point, indefinitely near any part of the surface, will be perpendicular to the surface and equal to $4\pi\rho$, if $\rho\omega$ be the quantity of matter on an element ω of the surface in the neighbourhood of the point. Coulomb's demonstration of this theorem may be found in a preceding paper in the Mathematical Journal, vol. iii. p. 74. He gives it himself, in his sixth memoir on Electricity (*Histoire de l'Académie,* 1788, p. 677), in connexion with an investigation of the theory of the proof plane in which, by an error that is readily rectified, he arrives at the result that a small insulated conducting disc, put in contact with an electrified conductor at any point, and then removed, carries with it as much electricity as lies on an element of the conductor at that point equal in area to the two faces of the disc ; the quantity actually removed being only half of this. This result, however, does not at all affect the experimental use which he makes of the proof plane, which is merely to find the ratios of the intensities at different points of a charged conductor. As the complete theory of this valuable instrument has not, so far as I am aware, been given in any English work, I annex the following remarkably

clear account of it, which is extracted from Pouillet's *Traité de Physique*: —" Quand le plan d'épreuve est tangent à une surface, il se confond avec l'élément qu'il touche, il prend en quelque sorte sa place relativement à l'électricité, ou plutôt il devient lui-même l'élément sur lequel la fluide se répand; ainsi, quand on retire ce plan, on fait la même chose que si l'on avait découpé sur la surface un élément de même épaisseur et de même étendue que lui, et qu'on l'eût enlevé pour le porter dans la balance sans qu'il perdît rien de l'électricité qui le couvre; une fois séparé de la surface, cet élément n'aurait plus dans ses différents points qu'une épaisseur électrique moitié moindre, puisque la fluide devrait se répandre pour en couvrir les deux faces. Ce principe posé, l'expérience n'exige plus que de l'habitude et de la dextérité : après avoir touché un point de la surface avec le plan d'épreuve, on l'apporte dans la balance, où il partage son électricité avec le disque de l'aiguille qui lui est égale, et l'on observe la force de torsion à une distance connue. On répète la même experience en touchant un autre point, et le rapport des forces de torsion est le rapport des repulsions électriques; on en prend la racine carrée pour avoir le rapport des épaisseurs. Ainsi le génie de Coulomb a donné en même temps aux mathématiciens la loi fondamentale suivant laquelle la matière électrique s'attire et se repousse; et aux physiciens une balance nouvelle, et des principes d'éxpérience au moyen desquels ils peuvent en quelque sorte sonder l'épaisseur de l'électricité sur tous les corps, et determiner les pressions qu'elle exerce sur les obstacles qui l'arrêtent."

To this explanation it should be added, that, when the proof plane is still very near the body to which it has been applied, the effect of mutual influence is such as to make the intensity be insensible at every point of the disc on the side next the conductor, and at each point of the conductor which is *under* the disc. It is only when the disc is removed to a considerable distance that the electricity spreads itself symmetrically on its two faces, and that the intensity at the point of the conductor to which it was applied, recovers its original value. It was the omission of this consideration that caused Coulomb to fall into the error alluded to above.

Note III.

This memoir of Green's has been unfortunately very little known, either in this country or on the continent. Some of the principal theorems in it have been re-discovered within the last few years, and published in the following works:—

Comptes Rendus for Feb. 11th, 1839, where part of the series of theorems is announced without demonstration, by Chasles.

Gauss's memoir on " General Theorems relating to Attractive and Repulsive Forces, varying inversely as the square of the distance," in the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre*, 1839, Leipsic, 1840. (Translations of this paper have been published in Taylor's Scientific Memoirs for April 1842, and in the Numbers of Liouville's Journal for July and August, 1842.)

Mathematical Journal, vol. iii. Feb. 1842, in a paper " On the Uniform Motion of Heat, &c."

Additions to the *Connaissance des Tems* for 1845 (published June 1842), where Chasles supplies demonstrations of the theorems which he had previously announced.

I should add that it was not till the beginning of the present year (1845) that I succeeded in meeting with Green's Essay. The allusion made to his name with reference to the word " potential" (Mathematical Journal, vol. iii. p. 190), was taken from a memoir of Murphy's, " On Definite

Integrals with Physical Applications," in the Cambridge Transactions, where a mistaken definition of that term, as used by Green, is given.

NOTE IV.

This theorem may be proved as follows:—

Let S be any closed surface, containing no part of the electrified bodies within it, which we may conceive to be described between A and B; let P be the component in the direction of the normal, of the resultant force at any point of the surface S, and let ds be an element of the surface at the same point. Then it may be easily proved (see Math.Journ.vol. iii. p. 204), that

$$\iint P \, ds = 0, \quad \cdots \quad \cdots \quad (a)$$

the integrations being extended over the entire surface. Now let S be supposed to consist of three parts; the portion a, of the surface of A; its projection β, on the interior surface of B; and the surface generated by the curved lines of projection. The value of P at each point of the latter portion of S will be nothing, since the tangent at any point of a line of projection is the direction of the force. Hence, if $[\iint P \, ds]$, and $(\iint P \, ds)$ denote the values of $\iint P \, ds$, for the portions a and β of S, the equation (a) becomes

$$[\iint P \, ds] + (\iint P \, ds) = 0.$$

But if ρ be the intensity of the distribution on the surface A or B, at any point, we have, by Coulomb's theorem,

$$\rho = \frac{P}{4\pi}.$$

Hence

$$[\iint \rho \, ds] + (\iint \rho \, ds) = 0,$$

which is the theorem quoted in the text.

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. vii. p. 526.]

May 4, 1854.—Colonel Sabine, R.A., Treas. and V.P., in the Chair.

THE following papers were read:—

1. " Account of Researches in Thermo-electricity." By Professor W. Thomson of Glasgow, F.R.S.

§ I. *On the Thermal Effects of Electric Currents in Unequally Heated Conductors.*

Theoretical considerations (communicated in December 1851 to the Royal Society of Edinburgh), founded on observations which had been made regarding the law of thermo-electric force in an unequally heated circuit of two metals, led me to the conclusion that an electric current must exercise a convective effect on heat in a homogeneous metallic conductor of which different parts are kept at different temperatures. A special application of the reasoning to the case of a compound circuit of copper and iron was made, and it is repeated here because of the illustration it affords of the mechanical principles on which the general reasoning is founded.

Becquerel discovered that if one junction of copper and iron, in a circuit of the two metals, be kept at an ordinary atmospheric temperature, while the other is raised gradually to a red or white heat, a current first sets from copper to iron through the hot junction, increasing in strength only as long as the temperature is below about 300° Cent.; and becoming feebler with farther elevations of temperature until it ceases, and a current actually sets in the contrary direction when a high red heat is attained. Many experimenters have professed themselves unable to verify this extraordinary discovery, but the description which M. Becquerel gives of his experiments leaves no room for the doubts which some have thrown upon his conclusion, and establishes the thermo-electric inversion between iron and copper, not as a singular case (extraordinary and unexpected as it appeared), but as a phænomenon to be looked for between any two metals, when tried through a sufficient range of temperature, especially any two which lie near one another in the thermo-electric series for ordinary temperatures. M. Regnault has verified M. Becquerel's conclusion so far, in finding that the strength of the current in a circuit of copper and iron wire did not increase sensibly for elevations of temperature above 240° Cent., and began to diminish when the temperature considerably exceeded this limit; but the actual inversion observed by M. Becquerel is required to show that the diminution of strength in the current is due to a real falling off in the electromotive force, and not to the increased resistance known to be produced by an elevation of temperature.

From Becquerel's discovery it follows that, for temperatures below a certain limit, which, for particular specimens of copper and iron wire, I have ascertained, by a mode of experimenting described below, to be 280° Cent., copper is on the negative side of iron in the thermo-electric series, and on the positive side for higher temperatures; and at the limiting temperature copper and iron are thermo-electrically neutral to one another. It follows, according to the general mechanical theory of thermo-electric currents referred to above, that electricity passing from copper to iron causes the absorption or the evolution of heat according as the temperature of the metals is below or above the neutral point; but neither evolution nor absorption of heat, if the temperature be precisely that of neutrality (a conclusion which I have already partially verified by experiment). Hence, if in a circuit of copper and iron, one junction be kept about 280°, that is, at the neutral temperature, and the other at any lower temperature, a thermo-electric current will set from copper to iron through the hot, and from iron to copper through the cold junction; causing the evolution of heat at the latter, and the raising of weights too if it be employed to work an electro-magnetic engine, but not causing the absorption of any heat at the hot junction. Hence there must be an absorption of heat at some part or parts of the circuit consisting solely of one metal or of the other, to an amount equivalent to the heat evolved at the cold junction, together with the thermal value of any mechanical effects produced in other parts of the circuit. The locality of this absorption can only

be where the temperatures of the single metals are non-uniform, since the thermal effect of a current in any homogeneous uniformly heated conductor is always an evolution of heat. Hence there must be on the whole an absorption of heat, caused by the current in passing from cold to hot in copper, and from hot to cold in iron. When a current is forced through the circuit against the thermo-electric force, the same reasoning establishes an evolution of heat to an amount equivalent to the sum of the heat that would be then taken in at the cold junction, and the value in heat of the energy spent by the agency (chemical or of any other kind) by which the electromotive force is applied. The aggregate reversible thermal effect, thus demonstrated to exist in the unequally heated portions of the two metals, might be produced in one of the metals alone, or (as appears more natural to suppose) it may be the sum or difference of effects experienced by the two. Adopting as a matter of form the latter supposition, without excluding the former possibility, we may assert that either there is absorption of heat by the current passing from hot to cold in the copper, and evolution, to a less extent, in the iron of the same circuit; or there is absorption of heat produced by the current from hot to cold in the iron, and evolution of heat to a less amount in the copper; or there must be absorption of heat in each metal, with the reverse effect in each case when the current is reversed. The reversible effect in a single metal of non-uniform temperature may be called a convection of heat; and to avoid cir-cumlocution, I shall express it, that the vitreous electricity carries heat with it, or that the specific heat of vitreous electricity is positive, when this convection is in the nominal "direction of the current," and I shall apply the same expressions to "resinous electricity" when the convection is against the nominal direction of the current. It is established then that one or other of the following three hypo-theses must be true :—

Vitreous electricity carries heat with it in an unequally heated conductor whether of copper or iron; but more in copper than in iron.

Or Resinous electricity carries heat with it in an unequally heated conductor whether of copper or iron; but more in iron than in copper.

Or Vitreous electricity carries heat with it in an unequally heated conductor of copper, and Resinous electricity carries heat with it in an unequally heated conductor of iron.

Immediately after communicating this theory to the Royal Society of Edinburgh, I commenced trying to ascertain by experiment which of the three hypotheses is the truth, as Theory with only thermo-electric data could not decide between them. I had a slight bias in favour of the first rather than the second, in consequence of the positiveness which, after Franklin, we habitually attribute to the vitreous electricity, and a very strong feeling of the improbability of the third. With the able and persevering exertions of my assistant, Mr. McFarlane, applied to the construction of various forms of ap-paratus and to assist me in conducting experiments, the research

has been carried on, with little intermission, for more than two years. Mr. Robert Davidson, Mr. Charles A. Smith, and other friends have also given much valuable assistance during the greater part of this time, in the different experimental investigations of which results are now laid before the Royal Society. Only nugatory results were obtained until recently from multiplied and varied experiments both on copper and iron conductors; but the theoretical anticipation was of such a nature that no want of experimental evidence could influence my conviction of its truth. About four months ago, by means of a new form of apparatus, I ascertained that *resinous electricity carries heat with it in an unequally heated iron conductor.* A similar equally sensitive arrangement showed no result for copper. The second hypothesis might then have been expected to hold; but to ascertain the truth with certainty I have continued ever since, getting an experiment on copper nearly every week with more and more sensitive arrangements, and at last, in two experiments, I have made out with certainty, that *vitreous electricity carries heat with it in an unequally heated copper conductor.* The third hypothesis is thus established: a most unexpected conclusion I am willing to confess.

I intend to continue the research, and I hope not only to ascertain the nature of the thermal effects in other metals, but to determine its amount in absolute measure in the most important cases, and to find how it varies, if at all, with the temperature; that is, to determine the character (positive or negative) and the value of the specific heat, varying or not with the temperature, of the unit of current electricity in various metals.

§ II. *On the Law of Thermo-electric Force in an unequally heated circuit of two Metals.*

A general relation between the specific heats of electricity in two different metals, and the law of thermo-electric force, in a circuit composed of them according to the temperatures of their junctions, was established in the communication to the Royal Society of Edinburgh referred to above, and was expressed by an equation* which may now be simplified by the thermometric assumption

$$t = \frac{J}{\mu};$$

(μ denoting Carnot's function, J Joule's equivalent, and t the temperature measured from an absolute zero, about $273\frac{1}{2}°$ Cent. below the freezing-point,) since this assumption defines a system of *thermometry in absolute measure,* which the experimental researches recently made by Mr. Joule and myself establish as not differing sensibly from the scale of the air-thermometer between ordinary limits. The equation, when so modified, takes the following form:—

$$F = J \left\{ \frac{\Theta_S}{S}(S - T) + \int_T^S \Im \left(1 - \frac{T}{t}\right) dt \right\},$$

where $\Im$ denotes the excess of the specific heat of electricity in the

* See Proceedings R.S.E. Dec. 1851, or Philosophical Magazine, 1852.

metal through which the current goes from cold to hot above the specific heat of the same electricity in the other metal, at the temperature t; F the thermo-electric force in the circuit when the two junctions are kept at the temperatures S and T respectively, of which the former is the higher; and Θ_S the amount of heat absorbed per unit of electricity crossing the hot junction. The following relation (similarly simplified in form) was also established:—

$$\vartheta = \frac{\Theta}{t} - \frac{d\Theta}{dt}.$$

These relations show how important it is towards the special object of determining the specific heats of electricity in metals, to investigate the law of electromotive force in various cases, and to determine the thermal effect of electricity in passing from one metal to another at various temperatures. Both of these objects of research are therefore included in the general investigation of the subject.

The only progress I have as yet made in the last-mentioned branch of the inquiry, has been to demonstrate experimentally that there is a cooling or heating effect produced by a current between copper and iron at an ordinary atmospheric temperature according as it passes from copper to iron or from iron to copper, in verification of a theoretical conclusion mentioned above: but I intend shortly to extend the verification of theory to a demonstration that reverse effects take place between those metals at a temperature above their neutral point of about 280° Cent.; and I hope also to be able to make determinations in absolute measure of the amount of the Peltier effect for a given strength of current between various pairs of metals.

With reference to laws of electromotive force in various cases, I have commenced by determining the order of several specimens of metals in the thermo-electric series, and have ascertained some very curious facts regarding varieties in this series which exist at different temperatures. In this I have only followed Becquerel's remarkable discovery, from which I had been led to the reasoning and experimental investigation regarding copper and iron described above. My way of experimenting has been to raise the temperature first of one junction as far as the circumstances admit, keeping the other cold, and then to raise the temperature of the other gradually, and watch the indications of a galvanometer during the whole process. When an inversion of the current is noticed, the changing temperature is brought back till the galvanometer shows no current; and then (by a process quite analogous to that followed by Mr. Joule and Dr. Lyon Playfair in ascertaining the temperature at which water is of maximum density) the temperatures of the two junctions are approximated, the galvanometer always being kept as near zero as possible. When the difference between any two temperatures on each side of the neutral point which give no current is not very great, their arithmetical mean will be the neutral temperature. A regular deviation of the mean temperature from the true neutral temperature is to be looked for with wide ranges, and a determination of it

would show the law according to which the difference of the specific heat of electricity in the two metals varies with the temperatures; but I have not even as yet ascertained with certainty the existence of such a deviation in any particular case. The following is a summary of the principal results I have already obtained in this department of the subject.

The metals tried being,—three platinum wires (P_1 the thickest, P_2 the thinnest, and P_3 one of intermediate thickness), brass wires (B), a lead wire (L'), slips of sheet lead (L), copper wires (C), and iron wire (I), I find that the specimens experimented on stand thermo-electrically at different temperatures in the order shown in the following Table, and explained in the heading by reference to bismuth and antimony, or to the terms " negative " and " positive " as often used :—

Temp. Cent.	Bismuth "Negative."					Antimony "Positive."
−20	...P_3...c ......P_2 ....P_1				......................I........	
0	...P_3...l' ......P_2 ......C ......P_1				..............................I........	
37	...P_3......b...{L'P_2}...... C...P_1				..........................I........	
64	...P_3............P_2... b... l' {CP_1}				....................I........	
130	...P_3............P_2............ {BP_1}...L... C......				...I........	
140	...P_3............P_2............. P_1...{BL} ......C ...I........					
280	...P_3............P_2............. P_1............ b... {CI}					
300	...P_3............P_2............. P_1............. b......I......C					

It must be added, by way of explanation, that the bracket enclosing the symbols of any two of the metallic specimens indicates that they are neutral to one another at the corresponding temperature, and the arrow-head below one of them shows the direction in which it is changing its place with reference to the other, in the series, as the temperature is raised. When there is any doubt as to a position as shown in the Table, the symbol of the metal is a small letter instead of a capital.

The rapidity with which copper changes its place among some of the other metals (the platinums and iron) is very remarkable. Brass also changes its place in the same direction possibly no less rapidly than copper; and lead changes its place also in the same direction but certainly less rapidly than brass, which after passing the thick platinum wire (P_1) at 130° Cent. passes the lead at 140°, the lead itself having probably passed the thick platinum at some temperature a little below 130°*.

* I have since found that it does pass the thick platinum, at the temperature 118°. [May 16, 1854.]

The conclusion as regards specific heats of electricity in the different metals, from the equation expressing thermo-electric force given above, is that the specific heat of vitreous electricity is greater in each metal passing another from left to right in the series as the temperature rises than in the metal it passes : thus in particular,—

The specific heat of vitreous electricity is greater in copper than in platinum or in iron ; greater in brass than in platinum or in lead ; and greater in lead than in platinum.

It is probable enough from the results regarding iron and copper mentioned above, that the specific heat of vitreous electricity is positive in brass ; very small positive, or else negative, in platinum, perhaps of about the same value as in iron. It will not be difficult to test these speculations either by direct experiment on the convective effects of electric currents in the different metals, or by comparative measurements of thermo-electric forces for various temperatures in circuits of the metals, and I trust to be able to do so before long.

§ III. *On Thermo-electricity in crystalline metals, and in metals in a state of mechanical strain.*

Having recently been occupied with an extension of the mechanical theory to the phænomena of thermo-electricity in crystalline metals, I have been led to experimental investigation on this branch of the subject. The difficulty of obtaining actual metallic crystals of considerable dimensions made it desirable to imitate crystalline structure in various ways. The analogies of the crystalline optical properties which have been observed in transparent solids, in a state of strain, and of the crystalline structure as regards magnetic induction which Dr. Tyndall's remarkable experiments show to be produced not only in bismuth but in wax, thick paste of flour, and " the pith of fresh rolls," by pressure, made it almost certain that pressure or tension on a mass of metal would give it the thermo-electric properties of a crystal. The only case which I have as yet had time to try, verifies this anticipation. I have found that copper wire stretched by a weight bears to similar copper wire unstretched, exactly the thermo-electric relation which Svanberg discovered in a bar cut equatorially from a crystal of bismuth or antimony compared with a bar cut axially from a crystal of the same metal. Thus I found that :—

If part of a circuit of copper wire be stretched by a considerable force and the remainder left in its natural condition, or stretched by a less force, and if either extremity of the stretched part be heated, *a current sets from the stretched to the unstretched part through the hot junction* : and if the wire be stretched and unstretched on the two sides of the heated part alternately, the current is reversed (as far as I have been able yet to test, instantaneously) with each change of the tension.

I intend to make similar experiments on other metallic wires ; also to try the effect of transverse as well as of longitudinal tension on slips of sheet metal *with their ends at different temperatures*, when placed longitudinally in an electric circuit ; and the effects of oblique tension on slips of metal similarly placed in a circuit, but kept with

their ends at the same temperature and their *lateral edges unequally heated.* I have no doubt of being able so to verify every thermo-electric characteristic of crystalline structure, in metals in a state of strain.

Glasgow College, March 30, 1854.

P.S. April 19, 1854.—I have today found by experiment that iron wire when stretched by a considerable force bears a thermo-electric relation to unstretched iron wire, the opposite of that which I had previously discovered in the case of copper wire; and I have ascertained that when the wire is alternately stretched and unstretched on the two sides of a heated part the current is reversed along with the change of tension, always passing from the unstretched to the stretched part, through the hot locality.

I hope before the end of the present Session to have a complete account of all the experiments of which the results are stated above, ready to communicate to the Royal Society.

2. "An Introductory Memoir upon Quantics." By Arthur Cayley, Esq., F.R.S.

The subject of Quantics is defined as the entire subject of rational and integral functions, and of the equations and loci to which these give rise, but the memoir relates principally to the functions called quantics; a quantic being in fact a rational and integral function, homogeneous in regard to a set of facients $(x, y..)$, or more generally homogeneous in regard to each of several such sets separately. A quantic of the degrees $m, m'..$ in the sets $(x, y..) (x', y'..)$ &c. is represented by a notation such as

$$(*)(x, y..)^m (x', y'..)^{m'}..),$$

where the mark $*$ is considered as indicative of the absolute generality of the quantic. The coefficients of the different terms of the quantic may be either mere numerical multiples of single letters or elements, such as $a, b, c..$, or else functions (in general rational and integral functions) of such elements; this explains the meaning of the expression the elements of a quantic. The theory leads to the discussion of the derivatives called covariants. Of these covariants a very general definition is given as follows, viz. considering the quantic $(*)(x, y..)^m (x', y'..)^{m'}..$, and selecting any two facients of the same set, *e. g.* the facients x, y, it is remarked that there is always an operation upon the elements tantamount as regards the quantic to the operation $x d_y$, viz. if we differentiate with respect to each element, multiply by proper functions of the elements and add, the result will be that obtained by differentiating with d_y and multiplying by x. And if the operation upon the elements tantamount to $x d_y$ is represented by $\{x d_y\}$, then writing down the series of operations

$$\{x d_y\} - x d_y, ..\{x' d'_y\} - x' d_{y'}, ..\&c.,$$

where x, y are considered as being successively replaced by every permutation of two different facients of the set $(x, y..)$, x', y' by every permutation of two different facients of the set $(x', y'..)$ &c.,

then it is clear that the quantic is reduced to zero by each of the operations of the entire system, but this property is not by any means confined to the quantic; and any function having the property in question, *i. e.* every function which is reduced to zero by each operation of the entire system, is said to be a covariant of the quantic. The definition is afterwards still further generalized, and its connection explained with the methods given, in the memoir ' On Linear Transformations,' Camb. and Dub. Math. Journal, Old Series, t. iv., and New Series, t. i., and the ' Mémoire sur les Hyperdéterminants,' Crelle, t. xxx., and some other theorems given in relation to the subject. The latter part of the memoir relates to the theory of the quantic $(*)(x, y)^m$, and to the number of and relations between the covariants, and as part of such theory to the beautiful law of reciprocity of MM. Sylvester and Hermite.

May 11.—The Earl of Rosse, President, in the Chair.

The following paper was read :—" On the relation of the Angular Aperture of the Object-Glasses of Compound Microscopes to their penetrating power and to Oblique Light." By J. W. Griffith, M.D., F.L.S.

The explanation given by Dr. Goring and others of the advantage of increased angular aperture in microscopic objective-glasses appears to the author to be correct, as applied to the case of opake objects, and accordingly his remarks in the present communication have reference to transparent objects only.

It is known that delicate markings on a transparent object, such as the valve of a *Gyrosigma*, may be rendered more distinctly visible by using an object-glass of large aperture, by bringing the mirror to one side, and by placing a central stop in the object-glass or the condenser or in both ; the increased distinctness produced in these several ways being due to the illumination of the object by oblique light. Experiment also shows that the degree of obliquity of the light requisite varies with the delicacy or fineness of the markings, being greater as these are more delicate ; so that the finest markings require the most oblique light which can possibly be obtained to render them evident, and the angular aperture of the object-glass must necessarily be proportionately large, otherwise none of these oblique rays could enter it.

If the parts of an object which refract the light are large in proportion to the power of the object-glass and of irregular form, they will refract a certain number of rays, so that these cannot enter the object-glass; hence certain parts will become dark, and will map out, as it were, in the image formed of the object, the structural peculiarities of the same. But if the parts are minute, of a curved form and approximatively symmetrical, they will act upon the light transmitted through them in the manner of lenses, and their luminous or dark appearance will vary according to the relation of the foci of these to that of the object-glass. Thus the parts of an object may appear dark and defined, from the refraction of the light out of the field of the microscope ; also, from the concentration or dispersion of por-

tions of the light by these parts, all the rays being admitted by the object-glass, or entering the field.

Another condition affecting distinctness consists in the relation which the luminousness or darkness of an object bears to that of the field or back ground upon which it is apparently situated.

The refraction of the light out of the field of the microscope or beyond the angle of aperture of the object-glass is the ordinary cause of the outlines of objects becoming visible; and in these cases, an increase of the angular aperture of the object-glass will impair their distinctness, because it will allow of the admission of those rays which would otherwise have been refracted from the field, and the margins will become more luminous and less contrasted with the luminous field.

The cause of the distinctness of an object by refraction when all or nearly all the rays enter the field of the microscope, may be investigated in a drop of oil immersed in water, or in a drop of milk, as illuminated by light reflected from an ordinary mirror. The refractive power of the globules is so great and their form such, that each acts as a minute spherical lens; and the parts within the margin will appear light or dark according to the relation of the focus of the little lens to that of the object-glass. Under an object-glass of small aperture and moderate power the outline will appear black, because the marginal rays do not enter the object-glass. If the object-glass be of sufficient aperture to admit these marginal rays, the black margin will disappear, and the little lens will only be distinguishable by the above focal relation. Its appearance under oblique light (thrown from all sides, as when the condenser and a central stop are used) will vary; but taking the case of extreme obliquity of the rays, the lens will only be visible by a luminous margin from reflexion, giving it a very beautiful annular appearance. Hence it is more distinct by direct, or slightly oblique, than by very oblique light.

But in certain objects, the irregularities of structure are of such extreme minuteness, or the difference of the refractive power of the various portions of the structure is so slight, that the course of the rays is but little altered by refraction on passing through them, and, under ordinary illumination, all the rays will enter the object-glass; neither are the rays accumulated into little cones or parcels, of sufficient intensity to map out the little light or dark spots in the field of the microscope, according to the relation of their foci with that of the object-glass.

Let us take the instance of an object with minute depressions on the surface, as the valve of a *Gyrosigma*. These are so minute, that when the light reflected from the ordinary mirror is used, the rays passing through the depressed and the undepressed portions, are not sufficiently refracted to cause either set to be excluded from the object-glass, consequently both sets will enter it. The slightly oblique and converging rays passing through a portion of the valve become separated into two sets, one passing through the thinner depressed portions, the other through the thicker and unde-

pressed portions: still both sets enter the object-glass. But on transmitting oblique light through the object, one set of the rays will be refracted so as not to enter the object-glass, whilst the other set will gain admission; thus the two parts, which have differently refracted the rays, will become distinct. If the markings were more delicate, or if the difference between the refractive power of the two portions of the valve were less, both sets would enter the object-glass. But on rendering the light still more oblique, one set would be again excluded by being refracted out of the field. Hence it is evident why the angular aperture of the object-glass must be larger as the markings are finer, or the difference between the refractive power of the two portions of tissue is less; because the obliquity of the light requisite will be very great to cause the exclusion of one set of the rays, and the other set will be too oblique to enter the object-glass unless it be of correspondingly large aperture. This is the explanation of the advantage of oblique light. It has no peculiar power of rendering objects distinct, as has sometimes been believed, and the following experiment, supposed to show such peculiar power, is really to be explained on different grounds. A piece of net, or some similar texture, is placed behind a hole made in a window-shutter, and when thus viewed, the fibres are not well seen; but when the texture is moved on one side, they become very distinctly visible, and this has been erroneously attributed to the illumination by oblique light; whereas the increased distinctness in the lateral position is owing principally to the circumstance that the object is then viewed on a dark instead of a white ground as in the first instance; although it is also true that in this position the oblique rays, being reflected in large numbers from the fibres into the eye, contribute to the distinct vision of the object when viewed as it then is upon a dark ground.

The most difficult point has been to explain, how an object-glass of large angular aperture will render markings evident, which were not visible under an object-glass of smaller aperture; because it would naturally be imagined that the larger aperture would admit both sets of rays, one of which was excluded by the object-glass of smaller aperture. The difficulty vanishes when it is recollected that the additional rays admitted by the object-glass of larger aperture are more oblique; hence one set of these rays will be refracted from the field of the microscope, whilst the other set will enter the object-glass and will illuminate the more highly refractive parts of the object; thus the two kinds of differently refractive structure become distinctly separated, one appearing dark, the other luminous; in fact, by means of the additional rays admitted by the larger aperture we illuminate more highly one part of the object whilst the illumination of the other is not increased. In short, the object is illuminated, first, by rays corresponding to those admitted by an object-glass of small aperture; and, secondly, by the additional rays admitted by the object-glass of larger aperture. The first set not being sufficiently oblique, no part of them is refracted beyond the angular aperture of the object-glass; the second, being more oblique,

are refracted out of the field by certain parts of the object and not by others, and thus contribute to render its different parts distinguishable by contrast of darkness and illumination. The first set of rays, by illuminating all parts of the object, tend to diminish this contrast, and consequently do not add to but impair the discriminative power of the object-glass for the fine markings of transparent objects, and accordingly these are rendered more distinctly visible by intercepting the less oblique rays by means of a central stop.

It has been here assumed that the oblique light requisite for the display of the markings upon objects is separated into two sets of rays by refraction; but the author observes that it might be questioned whether they are not separated by reflexion. There can be no doubt that the latter is not generally the case; perhaps the most important reason which may be assigned for this is, the considerable comparative breadth of the luminous portions of the valve of the *Gyrosigma* for instance. On transmitting unilateral light obliquely through the valve of an *Isthmia*, in which the depressions are large, in such manner that part of it is reflected by portions of them, it is easily seen how small the amount of reflected light is; and this because the surface of the depressions is curved, and thus the portions inclined at the requisite angle for reflexion are also very small. As the amount of light reflected is so small in this case, it would be inappreciable in that of the *Gyrosigma*, in which the depressions are so exceedingly minute. In fact, attention to this point affords a ready means of distinguishing whether an object is illuminated by reflexion or refraction.

The author next considers the relation of the *penetrating* power of an object-glass to its *defining* power. Penetrating power depends upon angular aperture, and consequently on oblique light. The question whether there be any essential difference between penetrating and defining power is best answered by experiment. If we take a fragment of the valve of an *Isthmia* and examine it under a high power of small aperture, all the parts are very distinctly seen by the ordinary light of the mirror; and the various depths of shadow of the different parts of the depressions and the undepressed portions render these also clearly distinguishable; and when an object-glass of very large aperture is used, the distinctness is rather impaired than improved. But if we examine a fragment of the valve of a *Gyrosigma*, and this requires an object-glass of large aperture to render the markings visible, no distinction of the various parts of the depressions and the undepressed portions is visible; all we see is, that the depressions as a whole are dark and the undepressed portions are luminous. Hence the *Isthmia* requires defining power, whilst the *Gyrosigma* requires penetrating power or large angle of aperture to exhibit the markings; yet the structures differ only in size. And there can be no doubt that if we could examine the valve of the *Gyrosigma* under a power as high relatively to the size of the depressions, as that under which we can examine the *Isthmia*, the same relations being preserved between the angle of aperture of the object-glass and the angular inclination of the refracted rays, the

various parts of the depressed and undepressed portions would be equally recognizable in both cases.

This is also true of fine lines scratched or etched on glass; for although the coarser lines upon glass micrometers are well seen with an object-glass of small aperture with good defining power and direct light, yet the finest lines upon Nobert's test-slide require penetrating power in the object-glass, and oblique light. Large angular aperture or *penetrating* power is but a very imperfect substitute for *defining* power—an important point which the author believes has not hitherto been noticed, and to which he would invite the earnest attention of object-glass makers.

The author concludes by observing that his remarks have been principally confined to one class of objects requiring penetrating power, viz. the valves of the Diatomaceæ. This has been done advisedly, because the scales of insects, which may be regarded as forming the type of the other class, involve considerations of a mixed kind, which would have tended to confuse the subject. The longitudinal ridges upon the scales of insects, in their relation to penetration, may be viewed as representing the undepressed portions of the valves of the Diatomaceæ; and the same explanation will apply to the visibility of both under various conditions. The transverse lines seen upon the scales are not indications of true structure; but their origin, as also that of the lines seen upon the valves of the Diatomaceæ, from circular or angular depressions, does not come within the conditions involved in the principle which it has been the object here to elucidate. It will suffice to say that the true structures producing the appearance of transverse markings upon the scales of insects are best resolved by small angular aperture and good definition.

It has been assumed also, that the markings upon the valves of the Diatomaceæ arise from depressions. This can be proved to be the case in the larger ones (*Isthmia*, &c.); and there is sufficient evidence to render it at least highly probable in the remainder. But this is an unessential point as regards the principle, and therefore it has not been dwelt upon.

ROYAL INSTITUTION OF GREAT BRITAIN.

May 19, 1854.—" On some Phænomena connected with the Motion of Liquids." By J. Tyndall, Esq., Ph.D., F.R.S., Professor of Natural Philosophy in the Royal Institution.

The lecturer commenced by referring to certain phænomena exhibited by liquids, and at variance with our commonly received notions as to their non-cohesive character. According to Donny, when the air has been as far as possible expelled from water by persistent boiling, such water possesses an extraordinary cohesive power, sufficient indeed to permit of its being heated to a temperature of 275° Fahr. without boiling. The adhesion of water thus prepared to the surface of a glass tube was shown experimentally; the force being sufficient to sustain a column of water of considerable height. The contractile force of a soap-bubble was referred to; and the lecturer passed on to the exhibition of the phænomena

resulting from the shock of two opposing liquid veins. In this case, though the forces are in opposite directions, motion is not annihilated; but the liquid, as first shown by Savart, spreads out so as to form a thin transparent film, the plane of which is at right angles to the direction of the jets. By varying the pressure on one side or the other, or by making the jets of different diameters, the plane film could be converted into a curved one, and sometimes actually caused to close, so as to form a pellucid sack. A cistern, situated at the top of the house and communicating by pipes with the lecture table, placed a considerable pressure at the disposal of the lecturer, and enabled him to exhibit in a striking manner the various phænomena described by Savart in his researches on the motion of liquids. A vein was caused to fall vertically upon a brass disc upwards of 3 inches in diameter: the liquid spread laterally on all sides and formed an umbrella-shaped pellicle of great size and beauty. With a disc of an inch in diameter, a pellicle of at least equal magnitude was formed. When a candle was placed underneath the curved sheet of water a singular effect was produced. The film above the candle was instantly dissipated; and on moving the candle, its motion was followed by a corresponding change of the aqueous surface. On turning a suitable cock so as to lessen the pressure, the curvature of the film became increased, until finally the molecular action of the water caused it to form a curve returning upon itself, and exhibiting the appearance of a large flask. When the film completely embraced the vertical stem which supported the brass disc, a change in the form of the liquid flask was observed, the latter became elongated, and was sometimes divided into two portions, one of which glided down the vertical stem and was broken at its base. When the jet was projected vertically upwards, large sheets were also obtained. The jet was also suffered to fall into small hollow cones of various apertures, and the shape of the liquid sheet received thereby some beautiful modifications. The enclosed sides of the hollow cone gave the liquid an ascending motion, which, combined with the action of gravity, caused the film to bend and constitute a vase-shaped surface of great beauty. The lecturer next referred to the constitution of a liquid vein; he had pointed out, some years ago, a simple mode of observing this constitution by means of the electric spark; this method corroborated the result before arrived at by Savart, that the lower portion of a liquid vein owes its turbidity to the fact of the mass being there reduced to drops, although the quickness with which they succeed each other gives the eye the impression of continuity. Savart's last experiments on this subject were repeated: a tube about 5 feet long and 2 inches wide had a perforated brass disc fixed at its lower extremity; the tube was filled with water, which, after it had become motionless, was permitted to issue from an orifice pierced in the centre of the disc. As the liquid escaped it gave birth to a succession of musical notes of sufficient intensity to be distinctly heard throughout the theatre. That these notes were not due to the motion imparted to the air by the descending drops of the liquid vein was proved, first, by intercepting the vein in its continuous portion, and secondly, by permitting it to

discharge itself into a vessel containing water, the orifice being caused to dip beneath the surface of the latter. In this case the mass of liquid was continuous, but the notes were nevertheless produced; thus showing that the vibrations which produce them must take place in the glass cylinder itself;—and corroborating the conclusions arrived at by Savart from his earliest experiments on this subject. The pitch of the note depends upon the height of the liquid column which produces it; and by attaching a tube of an inch in diameter, furnished with a perforated bottom, to a cylindrical vessel about 18 inches wide, and filling the whole with water, a note of long duration and of sensibly constant pitch was obtained.

The lecturer concluded with an experimental illustration of the total reflexion of light at the common surface of two media of different refractive indices. The tube communicating with the reservoir before referred to was fitted into the top of a small box, into one of the sides of which was fitted a glass tube $\frac{3}{4}$ of an inch wide and 5 inches long. The side of the box opposite to that through which the glass tube was introduced was of glass. Behind the box was placed a camera, by means of which the electric light could be condensed and caused to pass, first through the glass back of the box, and then through the tube in front, so as to form a white disc upon a screen held in the direct path of the light. When, however, the cock was turned so as to permit water to spout from the tube, the light on reaching the limiting surface of air and water was totally reflected, and seemed to be washed downward by the descending liquid, the latter being thereby caused to present a beautiful illuminated appearance.

X. *Intelligence and Miscellaneous Articles.*

ON THE PRINCIPLES WHICH GOVERN THE DISENGAGEMENT OF ELECTRICITY IN CHEMICAL ACTIONS. BY M. BECQUEREL.

ALL the questions relating to the evolution of electricity are exceedingly interesting in the physico-chemical sciences and their application to the arts and manufactures, seeing that they are of the same order as those which affect the production of heat during combustion.

The amount of electricity associated with the molecules of bodies is so great as to startle the imagination, as I showed in 1846. Unfortunately only an excessively small portion of this electricity can be collected, in consequence of the recomposition which takes place on the contact of the bodies. The efforts of physicists must be directed to the discovery of the means of preventing this recomposition, if they wish to furnish society with a motive power which may contend successfully with steam, producing much more varied effects, as it acts not only as a mechanical force, but also physically in the production of heat and light, and chemically in the decomposition of bodies and in effecting the combination of their elements.

Such was the end which I proposed to myself in 1823, when I produced my researches on the disengagement of electricity in chemical reactions, researches which enabled me to establish the principles

which govern this evolution, and by means of which I have succeeded in reproducing a certain number of mineral substances, and in recovering lead and silver from their respective ores, without any other employment of heat, except what is required for simple roasting, and even without the intervention of this amount of heat. This last investigation, which has taken several years of constant research, is now terminated, and I propose to lay its results before the Academy very shortly. The experiments have been made upon more than 30,000 kilogrammes of ores, not only from Mexico, but from various parts of the globe, and upon a sufficiently large scale to show whether the process is or is not practically applicable.

My account of the principles which govern the disengagement of electricity in chemical reactions has been generally adopted, but as my experiments have now been published some thirty years, I have thought it advisable, after so long a period, to go over these researches again. The apparatus employed was the depolarizing apparatus, which I have lately presented to the Academy, and of which I have given a description in this memoir.

I first attended to the electrical effects produced in the reaction of acid, alkaline or neutral solutions upon water, and upon one another, avoiding especially the effects of polarization resulting from products deposited upon the platinum plates intended for the transmission of the currents. I arrived at this result, that water is negative in relation to all acids and to saturated solutions of neutral salts and positive in relation to the alkalies; that in the reaction of acids upon each other, the most oxidizing acids are the most positive, and that the acids, in combination, transmit to their compounds their electro-positive properties, so that when mixed or combined, solutions of nitrates are positive in relation to sulphates, and sulphates in relation to chlorides. Hence we see why the intervention of nitromuriatic and nitric acids in galvanic batteries produces a greater evolution of electricity than the other acids, as was pointed out by me in 1827, when I described the pile in which each pair was formed of nitric acid, potash and platinum.

My experiments with the depolarizing apparatus have led me to this general fact: when water and several other neutral, acid or alkaline solutions are in contact (two and two) so as only to combine or mix very slowly, the electrical effect produced is the sum of the individual electrical effects produced at each surface of contact. This is in opposition to the principle advanced by Volta, namely, that when several solid or fluid substances are in contact one after the other, the electrical effects produced are the same as if the two extreme substances were immediately in contact.

The principle which I have just indicated leads to this result, that electrical circuits may be formed with liquids alone, as in fact I had shown to be possible in a memoir laid before the Academy in 1847. Similar circuits must also exist in living organized bodies, and hence we may conceive the possibility of the production of electro-chemical effects in organic tissues. In my memoir I have given two examples of currents of this nature, in the stems of vegetables during the movement of the sap, and in the tubers of the potato.

I have also shown how, with fluids and platinum or charcoal without an alkaline solution, we may succeed in obtaining piles with a constant current. I conclude my memoir by showing fully the evolution of electricity in the combustion of gases or of charcoal. I had already taken up this question in 1824, by means of the condensing electrometer and plates of platinum; a year afterwards M. Pouillet analysed these effects, especially those produced by the combustion of charcoal. Having already proved that during the contact of the flames with the plates of platinum thermo-electrical effects were produced, I again went over all my experiments in 1849, no longer with the electrometer, as I had found that flames were conductors of electricity of very low tension, as well as glass when heated even considerably below redness. It was on this occasion that my son Edmond made a series of experiments to show that hot air, and all gases heated to a suitable temperature, were conductors of electricity. It is extremely probable that all non-conducting bodies, when sufficiently heated, become conductors of electricity, just as they do when in a state of extreme tenuity.

I have been led to the discovery of a new property of platinum of which no idea has ever been entertained,—that of presenting different thermo-electrical properties at different temperatures. With these new means of action I have been able to show exactly that platinum, either in plate or wire, ought never to be employed in collecting the electricity of flames, seeing that it only produces thermo-electrical effects. With the same means of action, different from those hitherto employed, I have examined the evolution of electricity during combustion, and have been led to the same result as M. Pouillet; except that it is much more distinct, and its intensity may be augmented by assisting the combustion with nitre.

From the facts detailed in this memoir the following consequences may be deduced:—

1. In all chemical reactions electricity is disengaged.

2. In the reactions of acids or acid solutions with metals or alkaline solutions, the acids and acid solutions always take an excess of positive electricity, the metals and alkaline solutions a corresponding excess of negative electricity.

3. The disengagement of electricity during combustion is governed by the same principle, that is to say the combustible body disengages negative, the burning body positive electricity.

4. Decompositions produce inverse electrical effects.

5. There is no disengagement of electricity unless the two bodies are conductors of electricity; thus in the combination of a metal with dry oxygen, iodine or bromine, there is no production of electricity.

6. In the mixture of acids with water, or in their combination with that fluid, the water acts as a base, whilst it acts as an acid in relation to alkaline solutions.

7. Concentrated solutions of neutral salts act towards water in regard to the electrical effects produced in the same manner as acids with bases.

8. Acids when combining or mixing with each other behave so that

the most oxidizing acids are the most electro-positive; in combination with bases they appear to retain the same property, so that in the reaction or mixture of saturated solutions of two neutral salts, the nitrate is positive in relation to the sulphate, the sulphate to the phosphate, &c.

9. When several acid, neutral, or alkaline solutions are placed side by side so as to mix slowly, the electrical effects produced are the sum of the individual effects which take place at each surface of contact.

10. Contrary to Volta's opinion, an electric circuit, or rather a closed circuit, may be formed entirely with liquids in which a current of electricity circulates, and from which phænomena of decomposition and recomposition are produced, if there exist in this circuit corpuscles which are conductors of electricity. Living organized bodies present numerous examples of circuits of this kind, capable of giving rise to electro-chemical effects which have not yet been studied.—*Comptes Rendus,* April 24, 1854, p. 757.

METEOROLOGICAL OBSERVATIONS FOR MAY 1854.

Chiswick.—May 1. Rain.   2. Rain: low white clouds.   3. Cloudy.   4. Very fine.   5. Foggy: very fine.   6. Cloudy: uniformly overcast: slight frost at night.   7. Fine: rain.   8. Heavy rain.   9. Showery: thunder, with heavy showers.   10, 11. Cloudy.   12. Foggy: very fine: boisterous at night.   13. Fine.   14, 15. Very fine.   16. White clouds: partially overcast: very clear: frosty.   17. Uniform haze: clear, with very dry air: overcast.   18. Uniformly overcast: clear at night: frosty.   19. Clear and fine.   20. Cloudless: very fine: clear.   21. Cloudy and fine: rain.   22. Densely clouded: rain.   23. Rain: fine: very clear.   24. Heavy showers: clear at night: frosty.   25. Fine: showers, partly hail: clear.   26. Heavy clouds: very fine: heavy rain at night.   27. Heavy rain: hail-storm, with thunder and lightning quarter to five P.M.: clear at night.   28. Fine: cloudy: showery: heavy rain.   29. Rain.   30. Overcast: rain: cloudy.   31. Very fine throughout.

Mean temperature of the month ....…… 50°·07
Mean temperature of May 1853 .... 51 ·27
Mean temperature of May for the last twenty-eight years . 53 ·85
Average amount of rain in May .... 1·77 inch.

Boston.—May 1. Cloudy: rain A.M.   2. Cloudy: rain A.M. and P.M.   3. Cloudy: rain P.M.   4. Fine.   5. Cloudy: rain P.M.   6. Fine.   7. Cloudy: rain, with thunder and lightning A.M. and P.M.   8. Cloudy.   9. Cloudy: rain, with thunder and lightning A.M. and P.M.   10. Cloudy.   11. Cloudy: rain A.M.   12. Fine.   13. Cloudy.   14, 15. Fine.   16. Cloudy.   17. Fine.   18. Cloudy: rain A.M.   19, 20. Fine.   21. Cloudy.   22. Cloudy: rain A.M.   23. Rain A.M. and P.M.   24. Cloudy: rain A.M.   25. Fine.   26. Cloudy: rain A.M.   27. Cloudy: rain A.M. and P.M.   28. Fine.   29. Cloudy: rain A.M.   30. Cloudy: rain A.M. and P.M.   31. Cloudy.

Sandwick Manse, Orkney.—May 1. Clear A.M.: drops P.M.   2. Clear, fine A.M.: clear P.M.   3—5. Clear and fine A.M. and P.M.   6. Clear A.M.: rain P.M.   7. Clear and fine A.M. and P.M.   8. Clear, fine A.M.: cloudy P.M.   9, 10. Bright A.M.: showers P.M.   11. Bright A.M.: cloudy P.M.   12. Cloudy A.M. and P.M.   13. Bright A.M.: showers P.M.   14. Cloudy A.M.: drizzle, showers P.M.   15. Clear A.M. and P.M.   16. Drops A.M.: damp P.M.   17. Showers A.M. and P.M.   18. Clear A.M. and P.M.   19. Cloudy A.M. and P.M.   20, 21. Showers A.M.: cloudy P.M.   22. Cloudy A.M.: drops P.M.   23. Clear A.M. and P.M.   24. Bright A.M.: clear P.M.   25. Cloudy A.M.: rain P.M.   26—30. Cloudy A.M. and P.M.   31. Damp A.M.: drizzle P.M.

Mean temperature of May for twenty-seven previous years . 47°·98
Mean temperature of this month   .... 48 ·39
Mean temperature of May 1853 .... 49 ·67
Average quantity of rain in May for thirteen previous years. 1·68 inch.

by Mr. Veall, at BOSTON; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month. 1854. May.	Barometer Chiswick Max.	Chiswick Min.	Boston 8¼ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¼ p.m.	Thermometer Chiswick Max.	Chiswick Min.	Boston 8¾ a.m.	Orkney, Sandwick 9¼ a.m.	Orkney, Sandwick 8¼ p.m.	Wind Chiswick 1 p.m.	Wind Boston	Wind Orkney, Sandwick	Rain Chiswick	Rain Boston	Rain Orkney, Sandwick
1.	29·192	29·128	28·80	28·72	28·73	57	42	52·5	46	43	sw.	ssw.	se.	·16	·05	·06
2.	29·467	29·167	28·84	28·89	29·08	60	41	54	47½	44½	sw.	s.	se.	·02		·02
3.	29·482	29·437	29·05	29·26	29·41	60	37	54	51	43	sw.	sw.	se.	·01	·18	
4.	29·663	29·540	29·23	29·51	29·58	65	27	50	51	43	sw.	sw.	se.	·04	·15	
5. ☽	29·694	29·666	29·34	29·63	29·61	63	28	50·5	51	45	sw.	sw.	ese.			
6.	29·591	29·505	29·25	29·38	29·19	59	42	47·5	49	44½	s.	sw.	wsw.		·07	·26
7.	29·598	29·512	29·13	29·19	29·24	61	44	55	50	46	sw.	sw.	calm	·39		
8.	29·558	29·448	29·07	29·29	29·45	58	32	41	48	46	sw.	w.	calm	·62	·09	
9.	29·861	29·676	29·33	29·65	29·95	60	34	49	45	40	n.	n.	nnw.	·17	·11	
10.	30·039	29·992	29·70	29·96	29·68	58	41	48·5	50	43½	w.	s.	s.		·22	·02
11. ○	30·075	29·939	29·55	29·73	29·90	56	30	52	54	50	s.	n.	w.		·03	·13
12.	30·186	30·118	29·79	29·84	29·84	67	39	53	55	49	s.	nw.	w.			
13.	30·110	30·083	29·65	29·83	29·87	67	37	60	50	46	nw.	w.	w.			
14.	30·139	30·112	29·75	29·89	29·97	66	47	56	47	45½	ne.	n.	se.			·05
15.	30·120	30·097	29·74	30·18	30·24	61	43	53	52	44	n.	ne.	wsw.			·06
16.	30·116	30·125	29·84	30·12	30·04	69	30	49	49	49	ne.	w.	wnw.			·01
17.	30·163	30·032	29·77	29·93	30·00	56	40	49	49	44	ne.	ne.	nw.			·06
18.	30·140	30·040	29·66	30·11	30·18	64	30	51	46	44	ne.	n.	se.		·08	·02
19. ☾	30·229	30·199	29·87	30·14	29·97	71	30	55	50	49	ne.	n.	wsw.	·07		
20.	30·162	29·989	29·75	29·79	29·69	64	41	62	49	48½	s.	w.	se.	·19		
21.	29·884	29·706	29·43	29·54	29·53	62	47	60	50	47½	s.	s.	ese.	·30		·09
22.	29·581	29·492	29·16	29·43	29·29	62	44	55	50	49½	sw.	s.	ese.	·10	·10	
23.	29·654	29·514	29·14	29·38	29·55	62	38	61	52	46	sw.	s.	ese.	·08	·07	·21
24.	29·760	29·669	29·30	29·67	29·79	63	30	53	53½	48	sw.	sw.	wsw.	·12	·03	
25.	29·802	29·760	29·40	29·87	29·75	64	40	55	53½	47	sw.	sw.	e.	·48	·04	
26. ●	29·701	29·658	29·28	29·80	29·79	59	42	56	52½	49	sw.	sse.	ne.	·12	·11	·50
27.	29·774	29·639	29·27	29·72	29·66	62	37	54	53	48	sw.	sw.	e.	·42	·15	·03
28.	29·740	29·634	29·30	29·65	29·72	58	38	57	53	50	s.	ssw.	e.	·40	·42	
29.	29·628	29·576	29·24	29·78	29·86	65	41	56	52	50	s.	w.	ene.	·34	·03	
30.	29·839	29·765	29·35	29·90	29·99	67	36	54	53½	49	sw.	ssw.	n.		·07	·09
31.	30·128	29·948	29·54	30·02	30·05	71	40	52	48½	48	sw.	sw.				
Mean.	29·839	29·747	29·40	29·670	29·696	62·48	37·67	53·4	50·35	46·43				4·03	2·00	1·61

XI. *On a new Hygrometer or Dew-point Instrument.* *By* A.
Connell, *F.R.S.E., Professor of Chemistry in the University
of St. Andrews*[*].

[With a Plate.]

THIS instrument, like Professor Daniell's, produces the cool-
ing effect on the surface which is to exhibit the deposition
of dew, by the evaporation of æther; but it differs from it entirely
as regards the manner of producing that evaporation. It is in
no respect a cryophorus, but causes and maintains the evapora-
tion simply by the action of a small exhausting syringe. The
accompanying figure (Plate I. fig. 3) will explain the nature of
the arrangement.

A is a little round bottle of thin brass, well polished on the
outside, and capable of holding, when filled to the bottom of its
neck, half an ounce of liquid. Its diameter is about $1\frac{3}{20}$ths of
an inch. Its neck is $\frac{3}{4}$ inch high and about $\frac{3}{10}$ inch wide, and
spread out a little at top. The passage M which conducts into
the neck has throughout an internal diameter of $\frac{1}{4}$ inch, and
it is very essential that it should not be narrower than this.

B is a small mercurial thermometer, the bulb of which reaches
within $\frac{1}{2}$th of an inch of the bottom of the bottle, and the upper
part of the bulb is on a level with the surface of the liquid
contained in the bottle, or a little above that level. The bulb
should not be entirely immersed in the liquid. It is an
elongated cylinder about $\frac{2}{3}$rds of an inch long by about $\frac{1}{8}$th
of an inch in diameter. This thermometer has a small scale
attached to it, graduated according to both Fahrenheit and Cel-
sius' scale from 0° F. to 100° F. The stem of the thermometer

* Abridged from a Memoir read to the Royal Society of Edinburgh on
the 3rd of April, 1854, and to be published in their Transactions, vol. xxi.

is cemented at C into a little brass stopper fitted by grinding into the neck of the little bottle so as to be air-tight.   D is a small exhausting syringe of brass, the cylinder of which is 5 inches long by about $\frac{8}{10}$ths of an inch wide.   To prevent the heat produced by the friction attending the working of the piston being communicated to the bottle, the terminal valve of the syringe is inserted in a collar of ivory, NO.   The aperture in the portion of this ivory piece to which the valve is attached is at least $\frac{1}{15}$th of an inch in diameter, that of the remainder of it is $\frac{5}{20}$ths of an inch.   The syringe must effect its purpose of exhausting in as perfect a manner as an instrument of that size can accomplish.   FG is a clamp of brass capable of being attached by the screw horizontally to a window-sill in the position which it occupies in the figure, or vertically to a common table.   The syringe screws into this clamp at K by a projecting screw soldered to the former, when the clamp is screwed to a window-sill, as is the case when an observation is made at an open window; or this projecting screw is inserted at L when the clamp is fastened to a table, as is done when the experiment is made in a room.   In both cases the syringe itself occupies a horizontal position, and the little bottle and thermometer of course a vertical one; the projecting screw K should be so constructed as to cause the syringe to incline with the bottle a little downwards, so that the tendency of any æther to pass from the bottle into the syringe may be counteracted, and any trace of it which should pass into or be condensed in the syringe shall run back into the bottle.   The two surfaces of the portions of the clamp which grasp the fixture ought to be well roughened that their hold may be secure.

Let us suppose the instrument fixed at an open window on the sill.   We of course note the barometer and the temperature of the external air at the time; and it is convenient, if a wet-bulb thermometer happens to be at the window, to observe the amount of the cold of evaporation at the time, because it gives an idea of the point near which the dew-point may be expected, although of course such an observation is by no means essential.

A half-ounce measure graduated into drachms is then filled with good commercial sulphuric æther to the extent of three drachms of the liquid.   This is *slowly* and carefully poured by a proper lip in the measure into the bottle, the other hand being held so as to prevent any interference with this operation from any draft or wind, and the thermometer is immediately inserted in the bottle and the stopper properly fixed.   The process of exhaustion is then begun, at first slowly, by working the piston by one of the fingers of the right hand, so as to produce a gradual cooling agency and equable distribution of the effect, and

quickened somewhat as occasion may seem to require. In damp weather the working of the instrument can scarcely be too slow. One or two fingers of the left hand are held on the upper part of the syringe, both to aid the holding of the clamp and to be ready to obviate the consequences of any accidental detachment. When the cold of evaporation or other indications prepare us to expect a very dry air, it is best to begin from the first with a pretty quick action of the piston, so as to secure a good reduction, and there will be sufficient time for the spreading of the cold over all the necessary parts. The thermometer will in all cases be observed to begin to fall almost immediately; and generally speaking, according to the observations which I have carried on in St. Andrews for three or four months, the deposition of dew may be first observed on the exterior surface of the little brass bottle in from 1 to $1\frac{1}{2}$ minute. Sometimes, of course, when the air is drier than usual, longer working will be required; and when it is more moist, less time will be necessary.

It having been found that the æthereal vapour acted on the valves of the syringe when constructed of the usual oiled silk, they have since been formed, on the suggestion of the late Mr. Alexander Kemp, of gold-beater's leaf, four folds of that material being employed to form each valve.

By this arrangement, during the past winter, at an open window I have reduced the temperature of the æther in the little bottle, when I thought proper, from 20° to 30° below the temperature of the air. With the external air at 36° I have reduced the temperature to 8°. In a room of the temperature of 57°, I have effected a reduction of 42° below the temperature of the room. During the month of June of the present year I have had an opportunity of making some observations in London at higher temperatures and with a very dry atmosphere. Thus on one occasion, with the external air at $65\frac{2}{3}°$, the dew-point was found to be 40°, giving a reduction of $25\frac{2}{3}°$. On another, with a very dry air during a north-west wind, and the external thermometer in a sheltered position at $67\frac{1}{2}°$, the dew-point was 36°, showing a reduction of $31\frac{1}{2}°$. On other occasions, with the external thermometer at 68°, I have effected a reduction of $34\frac{1}{2}°$ below that temperature, the dew-point being 10° higher; and with the air at 77°, I found the dew-point at 56°.

Should it ever be thought that the reducing agency is not sufficient, we always have it in our power to augment the energy of the process by enlarging somewhat the syringe, say to $5\frac{1}{2}$ or 6 inches in length, by $\frac{9}{10}$ths of an inch in diameter. This, I doubt not, would give a considerable augmentation of power, and would not present an inconveniently large instrument; but

still I think it much better to continue it of the size I formerly mentioned, unless more extended observation shall show the propriety of further enlargement.

The expenditure of æther during the exhaustion is very small, being on an average about half a drachm, value from a halfpenny to a farthing. With the dew-point only 5° or 6° below the temperature of the air, I have obtained the required reduction with the expenditure of only one-tenth of a drachm. When the observation is completed, the residual æther is immediately poured back into a separate little bottle kept for the purpose, and well stopped, and it may be used again repeatedly, making it up each time to 3 drachms by adding fresh æther from another bottle in so far as necessary. I have used the same æther in this way for a week or two; but of course this ought not to be persevered in too long; and entirely fresh æther ought to be employed after a certain time, and preserved, as before, till no longer proper for use.

After the instrument has been used and the residual æther poured back, it is expedient to work the piston a few times backwards and forwards to expel residual æther or its vapour. The leather of the piston should be rubbed from time to time with olive oil, and care should be taken that the washers of the different screws do not become too dry. This is prevented by the occasional use of olive oil; and this is one of the first things to be looked to at any time the instrument may seem not to work well.

As there naturally will be a disposition to compare the indications of this instrument with those of other dew-point hygrometers, particularly with those of Professor Daniell's, I beg to offer a few observations with reference to such a comparison. Whilst all have agreed in admiring the beauty and elegance of Daniell's hygrometer as a philosophical instrument, the estimates which have been formed of the degree of accuracy of its results have not been quite uniform. Some meteorologists have taken it as a kind of standard of comparison, to which their calculations connected with wet-bulb investigations are made referable; and when they talk of the dew-point in any particular instance, they mean the point of saturation as it would be indicated by that hygrometer. On the other hand, it has not unfrequently been objected by others that its indications give the dew-point too high; a circumstance which has been explained on the view, that the evaporating surface of the æther communicates the cold impression to the corresponding zone of the containing bulb before the bulb of the thermometer is cooled in an equal degree; an effect which would not result from the employment of the instrument described in this paper, because the better conducting

power of metal than that of glass will lead to the more speedy diffusion over the whole bottle of the cold impression. Mr. John Adie, on comparing the results by Daniell's instrument with those obtained by Dalton's method of transference of cooled water from one vessel to another, which by most inquirers is admitted to be the most accurate of any, although far from being the least troublesome in point of execution, found the error occasionally to amount to $6\frac{1}{2}°$ above the result given by Dalton's method, and on an average of twenty-eight observations to reach $2°·9$. With a view to this question, I made a set of comparative experiments with Daniell's hygrometer, and that described in this paper, referring to Dalton's mode, as a trustworthy standard of comparison. The results will be found in a table inserted in my paper in the Transactions of the Royal Society of Edinburgh, and I will here merely mention that my conclusion from the whole was, that the tendency of Daniell's instrument was decidedly in excess, although not to so great an extent as found by Mr. Adie; whilst that of the instrument described in this paper was in the opposite direction; but that with neither did the error, on an average of sixteen or eighteen observations, reach $1°$ F. Having found, however, that twelve out of eighteen observations with Daniell's gave a result in excess, the extreme being $2\frac{1}{4}°$, I cannot admit the assumption of Daniell's instrument as a rigidly accurate standard of comparison, and would wish Dalton's method to be taken as the criterion; with reference to which I found the deficiency of the new instrument on an average of sixteen observations between $32°$ and $45°$, to be only $0·95°$ F., which is evidently quite consistent with the employment of the instrument as a dew-point hygrometer.

The mode of observation which I adopt is that usually followed in regard to dew-point instruments, viz. to observe as accurately as possible the first decided appearance of moisture on the surface of the brass ball, and note the temperature indicated by the immersed thermometer at the same instant; then to stop the process of exhaustion immediately, and again mark the temperature at the moment of disappearance of the moisture; and lastly, to take a mean of the two observations as the dew-point.

In constructing the instrument, it is essential that minute attention be paid to the various particulars which have been mentioned regarding the size of the different apertures and passages, the air-tightness of the several screws and of the thermometer stopper, the proper condition of the valves, the proper position of the bulb of the thermometer, &c. And it should be remembered that the apparatus ought not only to be well constructed at first, but should be *preserved* in good condition as regards the several points just mentioned. The oil of the

piston and valves should always be renewed before they get dry.

Although the various joinings must be air-tight, great care should be taken in screwing and unscrewing the bottle and ivory valve-piece that too great force shall not be used, as the fracture of the latter is otherwise risked. If at any time the screws get fixed, the best course is to tie cotton around them and moisten it well with olive oil, and leave them in this state for a couple of days till the washers get softened, when the joinings may be unscrewed with little risk of fracture. And it is expedient every time the instrument has been used, to loosen the screws a little till the next observation is to be made, when of course the necessary degree of tightness must again be cautiously given to them.

There is a slight modification of the form of the instrument, in which, instead of the ivory valve-piece, a collar of ivory is introduced into the neck of the bottle at P to cut off the communication of heat from the syringe. The terminal valve-piece of the syringe is in that case made of brass, and there is no risk of fracture of that portion of the instrument in screwing or unscrewing; but the bottle itself requires great care to be taken in handling it, as this point of junction may otherwise receive injury and become not air-tight. With this form of the instrument, and the external air at 74°, I effected one day in London a reduction to 31°, *i. e.* 43° below the temperature of the air, the dew-point being at 58°. Either modification, if in proper order, answers the purposes of the instrument sufficiently well*.

The instruments of Dœbereiner† and Dr. Cumming of Chester‡, with which I have become acquainted since I had that here described constructed, produce the evaporation of the æther in a different way, viz. by passing through it a current of air by a proper condensing apparatus. Regnault's apparatus for the dew-point also acts in the same way by the aid of an aspirator§.

It is thought that the instrument described in this paper will present some advantages in point of security from injury in travelling and in general use; the matter chiefly requiring some attention in this particular being the ivory intercepting portion, as already noticed.

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* Messrs. Kemp of Edinburgh have undertaken to prepare the instrument with bottles for æther, measure, &c., packed in a portable case.

† Gilbert's *Annalen*, tenth volume.

‡ Library of Useful Knowledge, Article Thermometer and Pyrometer.

§ See Ganot's *Traité de Physique*.

XII. *Reply to some Observations published by* Mr. Wilbraham *in the Philosophical Magazine,* vol. vii. p. 465, *on the Theory of Chances developed in* Professor Boole's *'Laws of Thought.'*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

CONTROVERSY is in every way so disagreeable to me, that it is with the most unfeigned reluctance I feel myself called upon to reply to the observations of Mr. Wilbraham inserted in the last Number of your Journal.

Mr. Wilbraham states that it is his object "to show that Professor Boole does in the greater number of questions relating to chances solvable by his method (or at least in those which are most difficult to treat by other methods), tacitly assume certain conditions expressible by algebraical equations, over and above the conditions expressed by the data of the problem, and to show how these conditions may be algebraically expressed." And in a subsequent passage he describes the procedure of that part of my work on the Laws of Thought which relates to the theory of probabilities, thus :—"In cases not determinable by ordinary algebra, his (Professor Boole's) system is this ; he takes a general indeterminate problem, applies to it particular assumptions not definitely stated in his book, but which may be shown, as I have done, to be implied in his method, and with these assumptions solves it ; that is to say, he solves a particular determinate case of an indeterminate problem, while his book may mislead the reader by making him suppose that it is the general problem which is being treated of." (Phil. Mag. vol. vii. pp. 465, 475.)

I fear that the impression produced upon the mind of any person not acquainted with my work by such statements as the above would be, that I have introduced in a covert manner assumptions of the existence of which I was ignorant, or of the recognition of which I was afraid. It may be therefore right for me to state that I have, in the chapter containing the demonstration of the general method for the solution of questions in probabilities (Laws of Thought, Chap. XVII.), explicitly stated the principles upon which that demonstration proceeds, and with equal explicitness deduced from them the algebraical equations upon which the solution depends. In the practical examples which are contained in the subsequent chapters, the rule to which the above-mentioned principles have led is applied without any reserve or addition whatever. To prove that particular assumptions not definitely stated in my book are employed, it ought, I conceive, to have been shown that the principles which I have expressly stated are insufficient for the conclusions which are

drawn from them.   But though I might, I think, justly complain
of the representations which Mr. Wilbraham has made, I very
gladly dismiss this part of the question, and desire to consider
simply whether Mr. Wilbraham's strictures affect in any way the
validity of the method which I have published.   Before entering
upon this inquiry, I would beg permission to state what is the
view which I have been led to take of the theory of probabilities
as a science.

It cannot, I think, be doubtful that the theory of probabilities
belongs to that class of sciences which are termed pure sciences.
Its fundamental idea or conception is that of probability.   From
this idea, from the definition of the *measure* of probability by
which it becomes associated with number, and from the laws
of thought with which it is connected through its having to do
with events capable of logical expression, flow the axioms and
first principles of the science.   I would refer in partial illustra-
tion of this view, to a remarkable paper by Professor Donkin,
published in this Journal (May 1851).   He has there announced
the following principle, and has shown that it leads at once to
(I believe) all the principles before recognized.   " If there be any
number of mutually exclusive hypotheses, $h_1, h_2, \ldots h_n$, of which
the probabilities relative to a particular state of information are
$p_1, p_2, \ldots p_n$, and if new information be given which changes the
probabilities of some of them, suppose of h_{m+1} and all that follow,
without having otherwise *any reference to the rest,* then the pro-
babilities of these latter have the same ratio to one another, after
the new information, that they had before ; that is,

$$p'_1 : p'_2 \ldots : p'_m = p_1 : p_2 \ldots : p_m,$$

where the accented letters denote the values after the new infor-
mation has been acquired."   I am not at present going to dis-
cuss this principle, but I adduce it as an instance of the general
position maintained, viz. that the ordinary doctrines and prin-
ciples of the theory of probabilities do run up into some more
general ones, the truth of which, when they are once stated, the
mind can hardly refuse to acknowledge, and which seem to be
involved in the very nature of expectation and of thought.   I go
on to observe, that such principles, if truly axiomatic, lead in
every pure science, and therefore in the theory of probabilities, to
a developed system of truth, or of methods for the attainment of
truth, which possess certain *invariable* characteristics never found
unimpaired where error has been permitted to enter.   These are,
mutual consistency, the property of verification wherever verifi-
cation is possible, continuity, and perhaps some other qualities
to which I cannot refer.   Now I propose to show, before I have
done, that the theory of probabilities does actually admit of this

kind of statement, progression and results. At present I merely offer these observations as preliminary to the question which I am called upon now to consider.

Mr. Wilbraham's remarks chiefly apply to two solutions of the following problem respectively published in this Journal by Mr. Cayley and myself. "The probabilities of two events A_1, A_2 are c_1 and c_2 respectively. The probability that if A_1 present itself another event E will accompany it is p_1, and the probability that if A_2 present itself E will accompany it is p_2. The event E cannot happen in the absence of A_1 and A_2, but of the connexion of the latter events nothing is known. Required the probability of E." (See also Laws of Thought, p. 321.) Representing A_1, A_2, and E by x, y, z respectively, the data of this problem are

Prob. $x = c_1$ Prob. $y = c_2$ Prob. $xz = c_1 p_1$ Prob. $yz = c_2 p_2$
    Prob. $z(1-x)(1-y) = 0$.

Mr. Wilbraham shows that both Mr. Cayley's solution and my own introduce two equations. To this I remark in passing, that there can be no objection so long as the equations in question are consequences of the laws of thought and expectation as applied to the actual data. Respecting the equations involved in my own solution, Mr. Wilbraham remarks :—"The second of these two assumed equations, though perfectly arbitrary, is perhaps not an unreasonable one. . . . . . I do not, however, see that it is a more reasonable or probable hypothesis than others that might be framed; for instance, than those assumed by Mr. Cayley in his memoir in this Magazine. But the first of these equations appears to me not only arbitrary but eminently anomalous." After this he deduces the equations which represent in a similar manner Mr. Cayley's hypotheses.

I should be reluctant to enter into any comparison of Mr. Cayley's solution and my own if the above remarks did not render it necessary to the interests of truth. It cannot be doubted that Mr. Cayley's solution is erroneous. Granting for a moment that both solutions involve hypotheses, there is this difference between them (a difference passed over in silence by Mr. Wilbraham), that Mr. Cayley's hypotheses lead to results absolutely inconsistent with the data—that my own hypotheses do not. One case easily tested is when we have $p_1 = 1$, and at the same time $p_2 = 0$. Another and more general case is when the constants are so related that we have either

$$c_1 p_1 + c_2(1-p_2) = 1,$$

or

$$c_2 p_2 + c_1(1-p_1) = 1.$$

I would refer on these points to a paper " On the conditions by

which the Solutions of Questions in the Theory of Probabilities are limited," which I forward for publication with this letter. On the other hand, there are no cases whatever in which the problem is solvable by other methods, which do not furnish a verification to the solution I have given.   Now I cannot but think that a cautious inquirer after truth, seeing that two hypotheses (still adopting Mr. Wilbraham's language), one of which appears to him "eminently anomalous," conduct to a solution which cannot by any known test be proved erroneous, while two other hypotheses, which appear to him "perhaps not unreasonable" (for this, Mr. Wilbraham's language already quoted implies with reference to Mr. Cayley's hypotheses), conduct to a solution which will not bear the test of examination, would be led to suspect that he had been judging of the reasonableness and of the anomalous character of hypotheses by some false standard.   Of course if a solution is erroneous, it need not to be argued that there must be error in the hypotheses by which it was obtained. But it is easy to show this directly.   If we apply the second of the equations representing Mr. Cayley's hypotheses to the particular case in which $p_1 = 1$, $p_2 = 0$, a case perfectly consistent with the character of the original data, it will be found to lead to the equation $c_1 c_2 = 0$, an equation *not* implied by those data in the particular case contemplated.   On the other hand, I affirm without hesitation that there is no case in which the equations deduced by Mr. Wilbraham from my method of solution can be proved to be erroneous.   They do not, indeed, represent "hypotheses," but they are legitimate deductions from the general principles upon which that method is founded, and it is to those principles directly that attention ought to be directed.

I would request your readers to observe that I do not offer the above remarks as affording any proof that the principles upon which my method is established are true, but only as conclusive that Mr. Wilbraham's objections against them, drawn from what to him appears to be the anomalous character of an equation to which they lead, are of no value whatever.   Nor is it difficult to see what is the source of the erroneous judgements, for erroneous I cannot but term them, which Mr. Wilbraham has been led to form.   It is in a principle, the influence of which appears to me to tinge the whole course of his speculations, that those events which in the language of the data appear as *simple events,* are the ultimate elements of consideration in the problem. These are the elements in terms of which he expresses his equations, overlooking the fact that it is by mere *convention* that such elements are presented as simple, and that the problem might have been expressed quite otherwise.   It cannot be too often repeated that the distinction of simple and compound is wholly

relative—that there is not and cannot be any kind of pre-emi-
nence among events founded merely upon the mode of their
expression. The neglect of this consideration makes truth to be
not merely the creature of language, but the creature of the
merest *accidents* of language.

The paper which I forward on the Conditions by which the
Solutions of Questions in the Theory of Probabilities are limited,
will be followed, should circumstances permit, by two others;
one containing a statement of the principles upon which my
method is founded, the other an analysis of its results considered
especially with reference to the question of the conditions of limi-
tation. It was my design to publish all these researches in a
single memoir. I have now determined to send them forth at
once, in the hope that when I shall have calmly stated my views,
I may with propriety leave the further discussion of them to
others.

<div style="text-align:center">

I am, Gentlemen,
Your most obedient Servant,

</div>

Lincoln, July 5, 1854.   **George Boole.**

XIII. *On the Conditions by which the Solutions of Questions in the Theory of Probabilities are limited.* By George Boole, *LL.D., Professor of Mathematics in Queen's College, Cork*[*].

SUPPOSE the following question in the theory of probabili-
ties to be given: "The probability that it rains on a given
day is p, the probability that it both rains and hails is q; required
the probability w that it neither rains nor hails." We know that
the data of this problem cannot represent a possible experience
unless p is equal to or greater than q. The absolute probability
of an event "rain," cannot be less than the probability of the
joint occurrence of that event and of another event "hail."
Again, we know that the probability w which we have to seek
cannot exceed $1-p$. The probability that it neither rains nor
hails cannot exceed the probability that it does not rain. Hence
the data of the problem are limited by the condition

$$p \gtreqless q;$$

and the probability sought, viz. w, by the condition

$$w \lesseqgtr 1-p.$$

If the former condition is not satisfied in the data, the problem
is not a *real* one. If the latter is not satisfied in the solution,
that solution may at once be pronounced to be incorrect. Con-
ditions of this nature are involved in almost every problem on

<div style="text-align:center">

[*] Communicated by the Author.

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chances in which the data are not the probabilities of simple in-
dependent events.   I propose in this paper to develope an easy
and general method of determining such conditions. This object
has been attempted in Chapter XIX. of my treatise on the ' Laws
of Thought.'   But the method there developed is somewhat dif-
ficult of application, and I am not sure that it is equally general
with the one which I am now about to explain.   I premise the
following proposition.

Proposition.—To eliminate any symbol of quantity x from any
system of inequations in the expression of which it is involved.

The general method will be best explained by an example.
Suppose it required to eliminate x from the inequations

$$x+y-z \gtreqless 0$$
$$3y-x-z \gtreqless 0$$
$$x-2y+z \gtreqless 0.$$

Reducing each of these inequations to a form in which the first
member shall be x, we have

$$x \gtreqless z-y$$
$$x \lesseqgtr 3y-z$$
$$x \gtreqless 2y-z.$$

From these equations it appears that x has for a superior
limit $3y-z$, and for inferior limits $z-y$ and $2y-z$.   As the
superior limit must in general exceed each of the inferior limits,
we have

$$3y-z \gtreqless z-y, \quad 3y-z \gtreqless 2y-z,$$

whence

$$2y \gtreqless z \quad y =\gtreqless 0.$$

And these are the only conditions which are independent of x.

The general rule would therefore be *to seek from the several
inequations the superior and inferior limits of* x, *and then to express
by new inequations the conditions that each superior limit shall be
equal to, or greater than, every inferior limit.*

If it is a condition that x is a positive quantity, then must
each superior limit be made $\gtreqless 0$; or we might add to the system
of inequations the inequation $x \gtreqless 0$, and apply the general rule.

When several quantities, as x, y, &c., are to be eliminated, we
can proceed by first eliminating x, then from all the inequations
which either result or remain eliminating y, and so on.   ·

It is obvious that the number of inequations obtained by the
elimination of a symbol may greatly exceed that of the inequa-
tions from which the elimination has been effected.

General proposition.—The probabilities of any events whose logical expression is known being represented by p, q, r.. respectively, required the conditions to which those quantities are subject.

Here also it may be well to commence with a particular case. I will take the problem already discussed in this Journal (Oct. 1853, Jan. 1854) by Mr. Cayley and myself. The elements of that problem may be thus expressed, w being the element sought in that discussion.

Prob. $x = c_1$ Prob. $y = c_2$ Prob. $xz = c_1 p_1$ Prob. $yz = c_2 p_2$

$$\text{Prob. } z = w \quad \text{Prob. } z(1-x)(1-y) = 0. \quad . \quad (1)$$

Here, according to the notation of the calculus of logic, Prob. xz denotes the probability of the occurrence of the events x and z together. Prob. $z(1-x)(1-y)$ denotes the probability of the occurrence of z conjointly with the absence of x and y, &c.

The events whose probabilities are given may all be resolved by logical development into disjunctive combinations of events, which do not admit of further resolution with reference to the same elements of distinction x, y, z. Thus

$$xz = xzy + xz(1-y)$$
$$x = xyz + xy(1-z) + x(1-y)z + (1-x)(1-y)(1-z).$$

And hence we have

$$\text{Prob. } xz = \text{Prob. } xyz + \text{Prob. } x(1-y)z, \quad . \quad . \quad (2)$$

and so on. Now assume

Prob. $xyz = \lambda$ Prob. $xy(1-z) = \mu$ Prob. $x(1-y)z = \nu$

Prob. $x(1-y)(1-z) = \rho$ Prob. $(1-x)yz = \sigma$

Prob. $(1-x)y(1-z) = \tau$ Prob. $(1-x)(1-y)(1-z) = \upsilon$.

These represent all the possible combinations of x, y and z, except $z(1-x)(1-y)$, which by the data is excluded.

The equation (2) gives, by virtue of (1),

$$\lambda + \nu = c_1 p_1;$$

and forming all similar equations furnished by the data, we have

$$\left. \begin{array}{l} \lambda + \mu + \nu + \rho = c_1 \\ \lambda + \mu + \sigma + \tau = c_2 \\ \lambda + \nu \qquad\;\; = c_1 p_1 \\ \lambda + \sigma \qquad\;\; = c_2 p_2 \\ \lambda + \nu + \sigma \quad = w \end{array} \right\} \quad . \quad . \quad . \quad . \quad (3)$$

to which we may add the necessary condition

$$\lambda + \mu + \nu + \rho + \sigma + \tau + \upsilon = 1. \quad . \quad . \quad (4)$$

Now the quantities λ, μ, ν, &c. are individually $\lessgtr 0$. More-over, they are subject to no other relations than the above.   Our object, then, is to seek the relations among w, c_1, c_2, c_1p_1, c_2p_2, which are necessary in order that the above conditions may be satisfied.

For this purpose we must, and the rule is of general applica-tion, determine as many of the quantities λ, μ, ν, &c. as we can in terms of w, c_1, c_2, &c., and make their expressions $\lessgtr 0$. These will furnish a part of the conditions sought. We must substitute the above expressions in the equations of the system (3) (4) which remain, and, supposing those residual equations to be n in num-ber, find from them the expressions of n more of the quantities λ, μ, ν in terms of the quantities which remain, and of the known quantities w, c_1, c_2, &c. We must make these expressions also $\lessgtr 0$, and from the inequations thus formed eliminate by the previous proposition such of the positive quantities λ, μ, ν as are still left. This will furnish the remaining conditions among the constants w, c_1, c_2, &c. In the steps of this process we shall have successively introduced all the conditions $\lambda \lessgtr 0$, $\mu \lessgtr 0$ ... $\nu \lessgtr 0$, and shall therefore have obtained all the equations con-necting the elements w, c_1, c_2, c_1p_1 and c_2p_2.

Thus, from the third, fourth, and fifth equations of (3), we find

$$\sigma = w - c_1p_1 \quad \nu = w - c_2p_2 \quad \lambda = c_1p_1 + c_2p_2 - w,$$
furnishing the conditions
$$w - c_1p_1 \lessgtr 0 \quad w - c_2p_2 \lessgtr 0 \quad c_1p_1 - c_2p_2 - w \lessgtr 0. \quad (5)$$
Substituting the values of σ, ν, and λ in the remaining equa-tions of (3) and (4), we find on transposition,

$$\mu + \rho = c_1(1 - p_1)$$
$$\mu + \tau = c_2(1 - p_2)$$
$$\mu + \rho + \tau + \nu = 1 - w.$$

Hence, selecting ρ, τ, and ν as the quantities to be determined, we have
$$\rho = c_1(1 - p_1) - \mu$$
$$\tau = c_2(1 - p_2) - \mu$$
$$\nu = 1 - w - c_1(1 - p_1) - c_2(1 - p_2) + \mu \, ;$$
whence, therefore,
$$c_1(1 - p_1) - \mu \lessgtr 0 \quad c_2(1 - p_2) - \mu \lessgtr 0$$
$$1 - w - c_1(1 - p_1) - c_2(1 - p_2) + \mu \lessgtr 0.$$
Or, in order to eliminate μ,
$$\mu \lesseqgtr c_1(1 - p_1) \quad \mu \lesseqgtr c_2(1 - p_2)$$
$$\mu \gtreqless c_1(1 - p_1) + c_2(1 - p_2) - 1 + w.$$

And hence

$$c_1(1-p_1) \gtreqless c_1(1-p_1) + c_2(1-p_2) - 1 + w$$
$$c_2(1-p_2) \gtreqless c_1(1-p_1) + c_2(1-p_2) - 1 + w;$$

or

$$\left. \begin{array}{c} w \lesseqgtr 1 - c_2(1-p_2) \\ w \lesseqgtr 1 - c_1(1-p_1) \end{array} \right\} \quad \ldots \quad \ldots \quad (6)$$

From the conditions (5) and (6), we see that w has for its lower limits, the expressions

$$c_1 p_1 \text{ and } c_2 p_2, \quad \ldots \quad \ldots \quad \ldots \quad (7)$$

and for its upper limits, the expressions

$$c_1 p_2 + c_2 p_2, \quad 1 - c_1(1-p_1) \text{ and } 1 - c_2(1-p_2). \quad \ldots \quad (8)$$

These are the conditions assigned in my treatise on the Laws of Thought, p. 325. They show, that if it is our object to determine Prob. z or w, the solution, to be a correct one, must lead us to a value of that quantity which shall exceed each of the values assigned in (7), and fall short of each of those assigned in (8). They show also that the data of the problem will only represent a possible experience when each of the values in (7) shall fall short of, or not exceed each of those in (8).

There is a class of problems characterized by the circumstance that the quantities λ, μ, ν . . are fewer in number than the equations in which they enter, which treated by this method lead to equations as well as inequations connecting the data with each other and with the probability sought. Whenever, too, the probability sought can be expressed as a linear function of the probabilities which are given, its actual expression will be determined by the above method, and it will agree with the result which would be assigned by the general method in probabilities (Laws of Thought, Chap. XVII.). To exemplify this, let us take the following problem (Ibid. p. 279).

Given Prob. $x = p$, Prob. $y = q$, Prob. $\big(x(1-y) + y(1-x)\big) = r$, to find the limits of Prob. xy or w.

Assume

Prob. $xy = \lambda$ Prob. $x(1-y) = \mu$ Prob. $(1-x)y = \nu$
Prob. $(1-x)(1-y) = \rho$.

Then we have as the conditions furnished by the data,

$$\left. \begin{array}{c} \lambda + \mu = p \\ \lambda + \nu = q \\ \mu + \nu = r \\ \lambda = w \\ \lambda + \mu + \nu + \rho = 1 \end{array} \right\} \quad \ldots \quad \ldots \quad (9)$$

From the three first equations and the last we find

$$\lambda = \frac{p+q-r}{2}, \quad \mu = \frac{p+r-q}{2}, \quad \nu = \frac{q+r-p}{2},$$

$$\rho = \frac{2-p-q-r}{2},$$

furnishing the conditions

$$p+q \gtreqless r, \quad p+r \gtreqless q, \quad q+r \gtreqless p, \quad p+q+r \lesseqgtr 2.$$

There still remains the fourth equation of the system (9), in the first member of which, substituting for λ its value, we find

$$w = \frac{p+q-r}{2},$$

the value of Prob. xy sought (Laws of Thought, p. 280).

There is a peculiarity in these " determinate " solutions to which I desire to advert. It is, that if in any series of observations the events referred to in the data occur with a frequency exactly proportional to their assigned probability, the event whose probability is sought will occur in the same series with a frequency exactly proportionate to its determined probability. For instance, in the problem just solved, if in n observations the events x, y, and $x(1-y)+y(1-x)$ occur exactly np, nq, and nr times respectively, the event xy will in the course of the same observations occur exactly $n\frac{p+q-r}{2}$ times. This is easily shown by substituting throughout the demonstration contained in Prop. 2, Nx for Prob. x, Ny for Prob. y, N applied to the expression of any class denoting the number of individuals contained in that class; and generally substituting *numbers* for *probabilities*. This change will not affect the truth of the equations. For instance, if we have

$$Nx = a \quad Nxy = b,$$

we shall have

$$Nx - Nxy = a - b,$$

or

$$Nx(1-y) = a - b,$$

and so on. I remark that this is a *peculiarity* of the above determinate solutions. If the probabilities of two independent events x and y are p and q respectively, the probability of their concurrence is pq; but we are not permitted to affirm, that if in n observations x occurs np times, and y occurs nq times, their concurrence will be observed exactly npq times.

When by the method of this chapter we have found the conditions of limitation of the solution of a question in the theory of probabilities, we can at once ascertain from those conditions

in what cases the problem becomes *determinate* in the sense above explained. Thus in the particular problem discussed in Proposition 2, since we have

$$\left.\begin{array}{ll} w \lesseqgtr c_1 p_1 + c_2 p_2 & w \lesseqgtr 1 - c_1(1-p_1) \quad w \lesseqgtr 1 - c_2(1-p_2) \\ w \gtreqqless c_1 p_1 & w \gtreqqless c_2 p_2 \end{array}\right\},(10)$$

it follows that whenever one of the upper limits of w becomes equal to one of the lower, the other conditions remaining satisfied, the problem becomes determinate. Thus, if we have $p_2 = 0$, we find from the above,

$$w \lesseqgtr c_1 p_1 \quad w \lesseqgtr 1 - c_1(1-p_1) \quad w \lesseqgtr 1 - c_2 \quad w \gtreqqless c_1 p_1.$$

Now as w cannot at the same time be both greater and less than $c_1 p_1$, it must be equal to $c_1 p_1$; the other conditions simply reducing to $1 - c_2 \gtreqqless c_1 p_1$. The solution, therefore, is

$$w = c_1 p_1,$$

the data being necessarily connected by the condition

$$1 - c_2 \gtreqqless c_1 p_1.$$

Let us apply to this case the solutions of the general question in probabilities respectively given by Mr. Cayley and myself. Mr. Cayley's solution is expressed by the quadratic equation

$$\left(1 - c_1(1-p_1) - w\right)\left(1 - c_2(1-p_2) - w\right) = (1-c_1)(1-c_2)(1-w).(11)$$

If we make $p_2 = 0$, it becomes

$$\left(1 - c_1(1-p_1) - w\right)(1 - c_2 - w) = (1-c_1)(1-c_2)(1-w),$$

and this equation is *not* satisfied when we make $w = c_1 p_1$. The solution which I have given is contained in the quadratic equation

$$(w - c_1 p_1)(w - c_2 p_2)(1-w) = \left(1 - c_1(1-p_1) - w\right)$$
$$\left(1 - c_2(1-p_2) - w\right)(c_1 p_1 + c_2 p_2 - w) ; \quad . \quad . \quad (12)$$

and this equation, on making $p_2 = 0$, is satisfied by the value $w = c_1 p_1$. The reader may examine for himself, and with exactly similar results, the class of cases in which the data happen to be connected by the relation

$$c_1 p_1 + c_2(1-p_2) = 1,$$

or by the relation

$$c_2 p_2 + c_1(1-p_1) = 1.$$

But there is another and more remarkable distinction to which I would advert. I have shown (Laws of Thought, p. 324), that in all cases in which the data of the above general problem are possible, the quadratic equation (12) furnishes one root, and only one, falling within the limits assigned by the method of this chapter. It is needless to remark that Mr. Cayley's equation

does not possess this characteristic. It may readily be shown that that equation will always furnish a single root satisfying the two conditions

$$w \lesseqgtr 1 - c_1(1 - p_1) \quad w \lesseqgtr 1 - c_2(1 - p_2) \, ;$$

but that the remaining three conditions assigned in (10) will not be satisfied by it unless certain other conditions, distinct from the conditions of possible experience, obtain.

And this leads me to notice, in the last place, a remarkable distinction, *à posteriori*, between unwarranted hypotheses in the solution of questions in the theory of probabilities, and axiomatic principles flowing out of the very idea and definition of probability, or sanctioned by the laws of thought. It is that the latter never impose, either upon the data or upon the solution, any limitations but those under which alone experience is possible, while the former do in general (always, I think, when the equation of a solution rises above the first degree) impose such limitations.

But these considerations only conduct us again to that general view of the theory of probabilities which is contained in my reply to the strictures of Mr. Wilbraham. They confirm, so far as they go, the doctrine already advanced, that its claim to rank among the pure sciences must rest upon the degree in which it satisfies the following conditions :—

1st. That the principles upon which its methods are founded should be of an axiomatic nature.

2nd. That they should lead to results capable of exact verification, wherever verification is possible.

3rd. That they should be capable of a systematic development consistent in all its parts and processes, and neither acknowledging nor imposing any limitations but those which exist in the nature of things.

Lincoln, July 6, 1854.

XIV. *On certain Questions relating to the Moon's Orbit, in reply to the objections of* Mr. Adams. *By* Professor Challis.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

BEFORE replying to the contents of the letter addressed to you by Mr. Adams on my new theorems relating to the moon's orbit, I think it right to state, in explanation of the origin of the discussion, that Mr. Adams came to the decision that the paper containing those theorems which I submitted to the Cambridge Philosophical Society was without merit, and

that the publication of it by an order of the Council of that body would bring credit neither to myself nor to the Society. If that opinion of the paper can be maintained, I shall admit that Mr. Adams had good reason for advising the Council not to print it. But I am well persuaded that the decision was rash, and made on mistaken grounds, and that my paper contained important additions to the lunar theory. I am therefore glad, as well for the sake of the interests of science as on my own account, that Mr. Adams has consented to publish his objections in the Philosophical Magazine. The article in the July Number gives me the opportunity I desired of vindicating my views. I shall discuss the objections *seriatim,* and in the order in which they occur, after premising some general remarks on the questions at issue.

It is well known that the theoretical determination of the motion of the moon's apse has been attended with difficulties. Newton gave, in the ninth section of the First Book of the *Principia,* methods of determining apsidal motion when the force is wholly central, but left nothing applicable to the circumstances of the moon's motion. The successors of Newton, who applied analysis to the lunar theory, finding that the process of approximation failed on starting from a fixed ellipse, altered the hypothesis of the approximation by *assuming* the apse to have a mean motion. This was done by "introducing" (to use Mr. Adams's expression) the quantity usually denoted by c. Laplace introduces this factor hypothetically, and refers to a subsequent verification. (*Théorie de la Lune,* vol. vii. sect. 4.) Plana, on the contrary, is led to it by the method of the variation of parameters. (*Théorie de Mouvement de la Lune,* Chap. II. § 3.) On the principle of the latter method I shall say a few words in the sequel, at present I am concerned with the *introducing* process. Now I think I may assert that mathematicians have felt that there is something unsatisfactory in this process; that it is a *pis aller,* to which recourse was had because no better method was discovered. Though it is undoubtedly legitimate and leads to true results, a method which would conduct *directly* to the form of the expression for the radius-vector to the first order of small quantities would seem to be more logical and more complete. I long since directed my attention to the discovery of such a method; and having at length remarked that the approximate solution of the problem of the moon's motion, as usually treated, rests on two distinct hypotheses, viz. a mean motion of the radius-vector and a mean motion of the apse, it occurred to me to attempt the investigation on the single hypothesis of a mean motion of the radius-vector. On doing so in the manner indicated in my communication to the April Number of the Philo-

sophical Magazine, I arrived at an equation which will be very
frequently referred to in the course of this discussion, viz.

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{m'r^2}{2a'^3} + C = 0. \quad \cdots \quad (C)$$

I saw at once that this equation, which, as I believe, had not
been previously noticed by any writer on the lunar theory, might
be employed in obtaining directly the true form of an approxi-
mate value of the radius-vector. The process for this purpose
will be considered in the course of the discussion.

I have a few remarks to make on statements contained in
page 28 of Mr. Adams's letter, which refer to views expressed in
the introductory part of my paper. My objections to the logic
of the process usually followed in the lunar theory are there
stated in these words:—" As it is certain that the mean motions
of the apse and node are consequences of the sun's disturbing
force, there must be some *direct* means of deducing them from
the dynamical equations of the motion. In fact, the method of
calculating the motion of the moon's node in the third book of
Newton's *Principia* shows, step by step, that the motion results
from the dynamical conditions of the problem, and in this respect
is logically more exact than the analytical method, which only
shows, on the hypothesis of such motion as Newton deduced,
that the dynamical equations are satisfied. There must be some
hiatus in the analytical method which requires to be filled up in
order that the logic of the lunar theory may be free from
reproach." My paper supplied this defect in the case of the
mean motion of the node as well as in that of the apse, and by
processes analogous to each other, as I shall take occasion to
show hereafter.

Again, with reference to the principle of introducing the quan-
tities *c* and *g*, I said that they were introduced *hypothetically,*
the apse and node being thereby supposed to have certain mean
motions; and that these hypotheses, suggested probably by
observation, are the real basis of the approximation in the usual
treatment of the lunar theory. The *symbolical* solution obtained
on these suppositions is, as Mr. Adams urges, proved to be cor-
rect by its satisfying the differential equations of motion; but
its applicability to a particular instance (as that of the moon's
motion) is not proved till the hypotheses on which the solution
was based are shown, by direct or indirect comparison with
observation, to hold good in that instance. This is the confir-
mation by observation that I spoke of. The solution which I
have proposed, being based on the single supposition that there is
a mean motion in longitude, requires only that we establish by
observation that the moon's motion satisfies this condition. I

said, moreover, that while my solution leads necessarily to the same numerical results as former methods, it adds something to our knowledge of the moon's orbit.

It will be proper to state here, also, my ideas respecting the constants introduced by integration, when the solution of the differential equations is only *approximate*. When an *exact* solution can be effected, I quite agree with Mr. Adams that, provided the proper number of constants be obtained, it matters not what is the process of integration and how it is suggested, simply for the reason that there is but one such solution, and its form is *unique*. Also the constants are arbitrary, and therefore entirely independent of each other. From an exact integral obtained in any manner, integrals applying to particular cases may be derived by assigning particular values to the arbitrary constants, or subjecting them to certain conditions. But when, as in the instance of the lunar theory, an exact solution is not attainable, that process is no longer possible, and recourse must be had to approximate solutions based on certain hypotheses. Now the form of an *approximate* solution entirely depends on the hypothesis on which the approximation proceeds, and may be different for every different hypothesis. Also each hypothesis requires a particular process of approximation which it is very important to discover. The constants introduced by the integration, if they should be the same in number as those of the exact solution, are not generally in the same degree arbitrary and independent of each other, because an integration performed on a certain hypothesis is equivalent to a particular case of the general integral. These constants may therefore have certain relations to each other, for the same reason that, in the common elliptic theory, the arbitrary constants of the general integral satisfy certain conditions in the particular cases of motion in a circle and a parabola. The process of approximation, if conducted strictly according to rule, will itself determine those relations between the constants which are appropriate to the circumstances defined by the hypothesis of the approximation. I am persuaded, from long consideration of the subject, that these principles are true, although they do not coincide with those laid down by Mr. Adams.

I proceed now to a particular discussion of Mr. Adams's objections. The first in order is that which begins at the top of page 30, to which an immediate answer may be given on the principles I have just explained. The fallacy of the argument consists in assuming that the constants a, ϵ, e, and ϖ are necessarily arbitrary and independent of each other, because the values of r and θ containing them satisfy the differential equations (1) and (2) at the bottom of page 29. Such reasoning is valid only in the case of the complete solution of *exact* equations. Also

the approximative process of integration made use of to obtain
the values of r and θ, does not decide whether or not the con-
stants are mutually related, because it is conducted in an *irregular*
manner.   After assuming that there is a mean motion of the
radius-vector, it makes the additional assumption of a mean
motion of the apse; whereas my method of integrating the equa-
tions (1) and (2) to the first approximation *deduces* the mean
motion of the apse from an assumed mean motion of the radius-
vector; and in the course of making this deduction, a certain
relation between the constants h and C (which may replace a and
e) is found which gives for e a special value.   The reasoning
which leads to this result will be defended in a subsequent part
of the discussion.

The assertions in the next paragraph (p. 30) respecting the
" variation " and " evection," are simply not true, the previous
values of r and θ belonging to my method of integration for the
same reason that they belong to the ordinary method.   After
obtaining the value of r to the first order of small quantities by
the process given in my communication to the April Number
(p. 281), I may use it in conjunction with the equations (1) and
(2) to obtain the values of r and θ to the second order of small
quantities precisely according to the usual method, the only dif-
ference being, that having *deduced* instead of *assuming* the form
of the first value of r, I am entitled to ascribe to e a special value.

In the latter part of the same paragraph Mr. Adams has
fallen into a misconception respecting Theorem I., which may
have arisen from the terms in which the theorem is enunciated.   In
my article in the April Number (p. 280), after obtaining the equa-
tion (C), I say, " this equation proves Theorem I."   I could not,
therefore, mean any other relation between the radius-vector and
the time than that expressed by the equation (C).   Mr. Adams's
objection to the correctness of the theorem will be met by enun-
ciating it with more precision as follows :—" All small quantities
of the second order being taken into account, the relation between
the radius-vector and the time in the moon's orbit, as expressed
by a differential equation of the first order, is the same as that
in an orbit described by a body acted upon by a force tending
to a fixed centre."

Mr. Adams then goes on to consider the reasoning by which
Theorems I. and II. are arrived at, and makes this remark :
" All this reasoning is based on the equation (C), the truth of
which, he (Prof. Challis) says, cannot be contested."   The rea-
soning to establish Theorem II. is certainly based on that equa-
tion, but Theorem I. is merely a verbal statement of what the
equation indicates.   Again, I said (Phil. Mag. for June, p. 430)
that the *proof* of the equation cannot be contested; and if I said

this untruly, why does not Mr. Adams contest it, or even allude to it? One sound argument against the reasoning employed to deduce the equation would suffice to settle the whole question. I can assure Mr. Adams, that while he has been unable to contest that reasoning, I have had no difficult task in discovering the fallacy or irrelevancy of every one of his attempts to nullify the equation.

The argument in the first paragraph of page 31 shows that the integral of the equation (C) gives the value of r only to the first order of small quantities. As this result perfectly accords with my views, I have no remark to make upon it. Neither in my paper nor in the Philosophical Magazine had I occasion to take into consideration the value of r to the second order of small quantities.

I come now to an argument which, among others, Mr. Adams communicated to me to justify his advising the Council of the Cambridge Philosophical Society to reject my paper. The argument then appeared to him to prove beyond a doubt that the equation (C) was erroneous : now he proposes it as a "test of the degree of accuracy to be attained by the use of that equation." By differentiating the equation (C) and dividing by $\dfrac{dr}{dt}$, there results

$$\frac{d^2r}{dt^2} - \frac{h^2}{r^3} + \frac{\mu}{r^2} - \frac{m'r}{2a'^3} = 0. \quad . \quad . \quad . \quad (D)$$

But the equations (1) and (2) already referred to, give

$$\frac{d^2r}{dt^2} - \frac{h^2}{r^3} + \frac{\mu}{r^2} - \frac{m'r}{2a'^3} - \frac{3m'a}{a'^3}\cos(\overline{2nt+\epsilon} - \overline{2n't+\epsilon'}) = 0, \quad (E)$$

which equation is certainly true to small quantities of the second order. Since, therefore, the equation (D) is not true, it was argued that the equation (C) from which it was deduced could not be true. If Mr. Adams had known the days of the Cambridge school exercises for mathematical honours, he would have got credit by propounding such an argument as this. As *opponent* he might have put the syllogism thus : if (C) be true, (D) is true, being "a strict deduction from (C)." But (D) is not true, because (E) is true. Therefore (C) is not true. *Ergo, cadit quæstio.* A clever *respondent* would, however, have immediately answered, *nego majorem* ; that is, he would not have admitted (D) to be a strict deduction from (C) by a retrograde step, unless (C) is deducible from the premises of the question *through* (D). But (D) is the antecedent of (C) only in case *the force is wholly central and the integration is exact,* which are not

the premises of the question. *Ergo, valet consequentia.*   It astonishes me that Mr. Adams did not perceive, that as the force is not wholly central, and the integration is approximate, the equation (E) is the legitimate antecedent of (C).   By multiplying the former by $\frac{dr}{dt}$, integrating, and omitting the quantity of the *third* order which the last term gives rise to, the equation (C) is immediately obtained.   In this method it is assumed that the value of $\frac{dr}{dt}$ contains a factor of the first order of small quantities, or, in other words, that the radius-vector oscillates in value to a small extent about a mean value, which might easily be shown to be a consequence of the supposition of a mean motion of the radius-vector in longitude.   My method of proving the equation (C) rests only on this latter supposition.

From this argument Mr. Adams does not, as before, conclude that the equation (C) is positively false, but that it is "unfitted for giving accurate information respecting the moon's orbit."   I must be permitted to express my great surprise that Mr. Adams could have written so unscientific a sentence as this.   What has a mathematician to consider but the truth of his equations ?   If they are false, they give no information ; if true, they necessarily give accurate information when rightly interrogated.   There is no intermediate species of equations.   My equation being proved to be true, must form an essential part of the lunar theory.   True results may be obtained without it by *pis aller* processes, but without it all that is true of the moon's orbit cannot be known.   It is clear to me that Mr. Adams is involved in a dilemma by having committed himself to the opinion that there was no merit whatever in my paper.   He cannot now admit that it contained an important equation which remained un-discovered from the days of Newton, and being unable to resist the evidence for the truth of the equation, he has recourse to the strange expedient of endeavouring to throw discredit upon the information it gives.

The arguments relating to Theorem I. conclude with the assertion that the equation (C) "would make the moon's apsidal distances to be *constant*," and a needless appeal to the Nautical Almanac : to which I reply, that the apsidal distances are not made constant by *deducing* the value of the radius-vector to the first order of small quantities from the equation (C), any more than they are made constant by *assuming* this same value.

We have now come to Theorem II. (p. 32). And here I cannot understand why Mr. Adams is solicitous to remove a diffi-culty which I found in extracting information from the equa-tion (C), since he is of opinion that this equation is unfitted to

give accurate information. I should have thought that he would have concluded the difficulty to be a confirmation of that opinion, and not regarded it as " purely imaginary." I, who have no doubt of the truth of my equation, may well believe that the difficulty is only apparent, and admits of explanation. Mr. Adams even volunteers to give an explanation, but, as might be expected, is not successful. He is equally unsuccessful in stating the difficulty. In the quotation from my article, inserted towards the bottom of page 32, the reference to the mean distance might have been omitted without detriment to the conclusion, as may be thus shown. Put a for r in the small term of equation (C), and we have

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{m'a^2}{2a'^3} + C = 0.$$

Now this equation informs us that the orbit is a fixed ellipse, and the force is wholly central and equal to $\frac{\mu}{r^2}$. The difficulty consists in its giving this information, although the effect of the disturbing force to small quantities of the second order has been taken into account. Also there are other ways in which the difficulty presents itself. By putting the equation under the form

$$dt = \frac{-r\,dr}{\sqrt{-Cr^2 - h^2 + 2\mu r + \frac{m'r^4}{2a'^3}}},$$

substituting a for r in the small term, and integrating, I find a relation between r and t, which, combined with the differential equation between r and θ, leads to apsidal motion, the amount of which is entirely false. Again, if in the small term $a + \overline{r-a}$ be put for r and be expanded to the *first* power of $r - a$, the result is an apsidal motion equal in amount to the known first approximation, but contrary in sign. These absurd and contradictory results plainly indicate that the equation (C), though it contains the disturbing force explicitly when a is put for r in the small term, contains it so as to give no information respecting the orbit. The only inference from this fact is, that the equation contains the disturbing force as a *factor*. Analysis supplies no other language by which the fact may be expressed. Mr. Adams asserts that this inference is "unwarranted," and "wholly unsupported by any proof." But here, as in other parts of the discussion, his assertions are stronger than his arguments. For if we now turn to the explanation he offers of the "supposed difficulty," we shall find that he leads us unwittingly to the very same conclusion respecting the existence of a factor.

After deducing from the equation (C), by making $\frac{dr}{dt}=0$, the equation for finding the apsidal distances, viz.

$$h^2-2\mu r+Cr^2-\frac{m'}{2a'^3}r^4=0, \quad . \quad . \quad . \quad (\text{F})$$

Mr. Adams goes on to obtain values of the apsidal distances by an approximate solution of this equation. The process he adopts, though conducted in an unusual manner, is nothing more than the ordinary Newtonian method, applied to approximate to two roots of an equation of *four* dimensions. On the *reason* for this process he is quite silent. The roots being put under the form $a(1\pm e)$ the resulting value of a is

$$\frac{\mu}{C}+\frac{m'\mu^3}{a'^3C^4},$$

which is known from independent considerations to be true. Mr. Adams might have added the resulting value of e^2, which is equally true, viz.

$$e^2=1-\frac{h^2C}{\mu^2}+\frac{m'\mu^2}{2a'^3C^3}.$$

This very remarkable result was first obtained in my paper, and ought alone to have saved it from unqualified condemnation.

Now, as to the *rationale* of the above process, it is known from the theory of equations, that since the term of highest dimensions contains a very small coefficient, the process would be quite illusory unless that coefficient be a factor of the equation. But it is a factor of the equation in case $h^2-2\mu r+Cr^2$ be a complete square, that is, if $h^2C=\mu^2$. For then the equation takes the form

$$C\left(r-\frac{\mu}{C}\right)^2-\frac{m'}{2a'^3}r^4=0,$$

and, putting $\frac{\mu}{C}+f\delta$ for r, may be transformed into

$$Cf^2\delta^2-\frac{m'}{2a'^3}\left(\frac{\mu}{C}+f\delta\right)^4=0.$$

Hence the required condition is satisfied if the unknown quantity f be such that $f^2=\frac{m'}{2a'^3}$.

All that remains to complete this reasoning is to show that no result applicable to the moon's orbit can be derived from the equation (F), *unless it be treated as an equation of four dimensions.* Here, again, Mr. Adams comes to my assistance. For he has shown, that if the equation be put under the form

$$h^2-2\mu r+\left(C-\frac{m'a^2}{2a'^3}\right)r^2=0,$$

and be solved as a quadratic, the resulting value of a is

$$\frac{\mu}{C} + \frac{m'\mu^3}{2a'^3C^4},$$

which is not true. If, without altering the form of (F), a be put for r in the small term, and the equation be solved as a quadratic, the value of a is found to be $\frac{\mu}{C}$. These contradictory and false results show that the apsidal distances can be obtained only by solving (F) as a biquadratic, and consequently that the equation contains the disturbing force as a factor. Mr. Adams is not happy in the inference he draws (in the first paragraph of page 34) respecting the different values of a given by his two processes. Plainly he had not bestowed on the subject all the consideration it demands.

In reply to the argument in the next paragraph, that "the required condition would be satisfied if h^2C differed from μ^2 by any quantity involving the disturbing force as a factor," it is enough to say that the equation $h^2C = \mu^2$ is *sufficient*, and that it would be a violation of the principles of analytical reasoning to introduce gratuitously a quantity to determine which there are no conditions. It is true that on carrying the approximation to small quantities of a higher order, it will be found that $h^2C - \mu^2 = $ a quantity containing the disturbing force as a factor, but at the same time the approximation itself determines the form and value of this quantity.

The next attack on the equation (C) is made on the principle, that an approximate equation, formed so as to include all quantities of the second order, may be proved to be false by reference to quantities of the third order which have been neglected in deducing it. No one, I think, ever heard of such a principle before. To state this argument is to refute it.

In the first paragraph of page 35 it is contended that the relations between r, θ and t, in page 281 of my article in the April Number, do not satisfy the equations (B) and (C) from which they are derived. They satisfy these equations by taking account of a term involving the sun's longitude, which by the integration rises to the *third* order, and which on that account was omitted. Surely there was no necessity to bring forward such an argument as this.

The three differential equations which are the basis of the whole of the analytical reasoning, were formed on the supposition that the ordinate z is small compared to the radius-vector r. In Titan's orbit, z may be very nearly the half of r, for which reason the theory does not apply to that body. In fact, it cannot be tested by reference to a system of satellites, the mutual

disturbances of which may countervail the disturbing force of the sun.

I come now to an argument in which I admit that Mr. Adams has taken a right view. I attempted by reasoning conducted *verbally*, to confirm the results of an analysis which stood in need of no such confirmation. If that reasoning had been good, it would have shown that the eccentricity is proportional to the *square* of the ratio of the periodic times, and not, as stated in Theorem II., simply proportional to that ratio. I am therefore thankful to Mr. Adams for proving that this verbal reasoning was in fault.

The assertion in page 36, that "if the disturbing force were increased the total fluctuation in the value of the radius-vector would be the same as before," begs the question at issue. So also does the "fatal objection" which follows it. To determine generally the orbit described under given initial circumstances, it would be necessary to commence the approximation on the single supposition that the ratio of the moon's radius-vector to that of the sun is small, a problem which has never been attempted. To this supposition is always added that of a mean motion of the radius-vector differing little from the true motion. I have shown that this latter supposition conducts to a special value of the eccentricity depending on the ratio of the mean motions of the moon and sun, and have thus given at least a *negative* proof that no part of the eccentricity would be constant if that particular relation did not exist. To prove the same thing positively would require the solution of the general problem above mentioned. It would be waste of time to say more in defence of what I only stated to be a probability.

As Mr. Adams appeals to the method of the variation of parameters in support of the last argument, I shall take the occasion to say a few words on the principle of this method. In the first place, I remark that it has been applied in the lunar theory only on the hypothesis of a mean motion of the radius-vector, and has decided on that hypothesis that the non-periodic part of the eccentricity is constant; but not having been applied to the more general problem, it has not decided that there would be a constant part of the eccentricity under *all* initial circumstances. Again, the method of the variation of parameters is simply a process of integration applicable to differential equations of a certain form, and requiring the same rules of treatment whether the differential equations be approximate or exact. Consequently in the former case as well as in the other, the constants introduced by the integration necessarily present themselves as independent of each other and arbitrary in value; but in the case of an approximate solution, they are not thus *proved*

to be arbitrary and independent. For considerations quite separate from the process of integration by the variation of parameters, and arising out of the hypothesis of the approximation, may show that those constants may have certain analytical expressions or values, and certain relations to each other. Thus the *process of integration* by the method in question proves that there is a constant part of the radius-vector; *the process of approximation* finds for this part the expression $a\left(1 - \dfrac{m^2}{6} + \dfrac{e^2}{2}\right)$. So by the variation of parameters the eccentricity is proved to have a constant part; but that process is incapable of arriving at such an equation as $e^2 = 1 - \dfrac{h^2 C}{\mu^2} + \dfrac{m'\mu^2}{2a'^3 C^3}$, much less of deciding whether h and C have a relation to each other. All that is *proved* by the method of the variation of parameters must be true; but other results not inconsistent with its indications, and not deducible by it, may also be true. I contend, therefore, that it affords no handle for an argument against my views.

I have now met, I think successfully, the whole *congeries* of Mr. Adams's arguments, the number of which does not make up for their want of force. By attacking an equation, the evidence for which is irresistible, Mr. Adams took up a false position which it was impossible to maintain. Nothing is now wanting to establish fully the solution of the lunar problem, of which I have indicated the initial steps, than to show, by carrying the approximation further, that it explains the variation and evection, and gives more approximate values of the eccentricity and the mean motion of the apse. I have not yet had time to do this, but I feel confident that the solution, if rightly conducted, will bear this test.

In concluding this long letter, I propose to give the method of determining, by a direct process to the first approximation, the mean motion of the moon's node, which, as before intimated, was contained in my paper. The differential equations of the motion are,

$$\frac{d^2x}{dt^2} + \frac{\mu x}{r^3} - \frac{m'x}{2a'^3} - \frac{3m'r}{2a'^3}\cos(\theta - 2\theta') = 0$$

$$\frac{d^2y}{dt^2} + \frac{\mu y}{r^3} - \frac{m'y}{2a'^3} + \frac{3m'r}{2a'^3}\sin(\theta - 2\theta') = 0$$

$$\frac{d^2z}{dt^2} + \frac{\mu z}{r^3} + \frac{m'z}{a'^3} = 0.$$

The last term in the third equation is a small quantity of the third order on account of the small ratio of z to r. If, therefore, we neglect this term, and suppose, as Newton does, the orbit to be a circle, we have for determining the period of the moon's

oscillations in latitude the equation

$$\frac{d^2z}{dt^2} + \frac{\mu z}{a^3} = 0.$$

The time of an oscillation is therefore $\dfrac{2\pi a^{\frac{3}{2}}}{\sqrt{\mu}}$. Also the known approximation to the moon's periodic time, as derived from the first two equations, is

$$\frac{2\pi a^{\frac{3}{2}}}{\sqrt{\mu}}\Big(1 + \frac{m'a^3}{4\mu a'^3}\Big).$$

As this period is longer than the period of oscillation in latitude, the node regresses, and the amount of regression in one lunation is the arc which the moon describes in the difference of the periods; that is, putting p and P for the periodic times of the moon and sun, the regression is $2\pi \times \dfrac{p^2}{4\mathrm{P}^2}$ nearly. This, however, as is well known, is a false result, and it is necessary, therefore, to retrace our steps. The last term of the third equation ought not to have been omitted, if that equation contains a small quantity of the first order as a *factor*, which will be found to be the case. For, putting a for r in the third equation, and integrating inclusively of the last term, we have

$$z = \mathrm{A} \cos (\mathrm{N}t + \mathrm{B}),$$

which shows that the small factor of the equation is A, the maximum value of z. The period of the moon's oscillation in latitude is nearly

$$\frac{2\pi}{\mathrm{N}}, \text{ or } \frac{2\pi a^{\frac{3}{2}}}{\sqrt{\mu}}\Big(1 - \frac{m'a^3}{2\mu a'^3}\Big),$$

which is less than the moon's period by

$$\frac{2\pi a^{\frac{3}{2}}}{\sqrt{\mu}} \times \frac{3m'a^3}{4\mu a'^3} \text{ nearly.}$$

Hence in one revolution of the moon the node regresses through the arc $2\pi \times \dfrac{3p^2}{4\mathrm{P}^2}$, which is the known first approximation. Consequently,

$$z = \mathrm{A} \cos\Big(\frac{2\pi t}{p}\Big(1 + \frac{3p^2}{4\mathrm{P}^2}\Big) + \mathrm{B}\Big),$$

the result it was required to obtain. This investigation presents several points of analogy with that for finding the mean motion of the apse.

<div align="center">I am, Gentlemen,</div>

<div align="right">Your obedient Servant,</div>

<div align="right">J. Challis.</div>

Cambridge Observatory,
    July 18, 1854.

XV. *Further researches on the Methods of Preserving the Sensitiveness of Collodion Plates.* By JOHN SPILLER *and* WILLIAM CROOKES*.

THE object of our former paper in the Philosophical Magazine for May, was to draw attention to the principle of preserving the collodion surface moist by taking advantage of the deliquescent character of certain bodies; and we preferred that method of communicating our discovery, as, although theoretically correct, it was not sufficiently developed to warrant our laying it before practical photographers, but required further experiments by which the comparative merits of the different substances at our disposal could be determined.

Having decided against the use of nitrate of zinc, we tried other substances, and among the rest the acetate of potash; but although by its use we succeeded in obtaining very good results, yet the sparing solubility of acetate of silver necessitates so many precautions, that we determined if possible to find an equally efficacious salt among the nitrates.

On giving the nitrate of magnesia a further trial, under circumstances which later experience had shown to be necessary, our former difficulties vanished, and we are now enabled to communicate to the Society the following process, which, in our opinion, scarcely admits of an improvement.

The plate coated with collodion in the usual manner is to be rendered sensitive in a 30-grain nitrate of silver bath, in which it should remain rather longer than is generally considered necessary (about five minutes), it must then be slightly drained and immersed in a second bath, consisting of—

Nitrate of magnesia . .	4 ounces.
Nitrate of silver . . .	12 grains.
Glacial acetic acid . . .	1 drachm.
Water	12 ounces.

and there left for about five minutes, then removed and placed in a vertical position on blotting-paper, until all the surface-moisture has drained off and been absorbed; this generally takes about half an hour, and they may then be packed away in any convenient box until required for use.

Not only is the sensitiveness unimpaired by this treatment, but we think, on the contrary, that it is slightly increased; instantaneous negatives have been taken on plates which had been prepared some days previously. We are not yet in a position to give the length of time that may elapse between the preparation of the plate and development of the picture; such expe-

riments necessarily require a more lengthened period than we have at present been able to give, but as long as they have yet been kept (upwards of three weeks), there has been no appearance of deterioration.

Before the development, we find it advisable to moisten the collodion film by immersion in the silver-bath for about half a minute, as otherwise the pyrogallic acid or iron solution would not flow evenly over the plate.   The fixing, &c. is of course conducted as usual.

It will be as well to draw attention to a few points which, although not absolutely essential, may possibly be found useful in practice.   The glass plates should be cleaned with more care than is necessary when they are to be used immediately; we have found strong nitric acid applied with a tooth-brush most convenient.   With regard to the collodion, we have tried very many different samples, and with tolerably uniform success. The greater number of our experiments have been made with a tolerably thick collodion, the alcohol and æther of which were in the proportion of 1 : 2, made sensitive with four grains of iodide and half a grain of bromide of ammonium to the fluid ounce. We have also employed a collodion containing iodide and bromide of cadmium with good success.

Of the 30-grain silver solution for exciting the plate we have only to recommend the use of acetic instead of nitric acid, to give the bath that faintly acid reaction which is by some operators considered desirable.

There are one or two circumstances to be attended to in the preparation of the magnesia-bath.   Commercial fused nitrate of magnesia is very liable to contain chlorine, and also to have an alkaline reaction on account of the fusion being carried too far. Of course the quantities of acetic acid and nitrate of silver given in the formula for the bath are on the supposition that the nitrate of magnesia is pure; if this be not the case, it should be rendered perfectly neutral with acetic acid, the chlorine exactly precipitated with nitrate of silver, and then the proper amounts of acid and silver added.   However, if the impurities are very considerable, it will be safer to reject the salt at once.   This bath will keep in good order for a long time; the only point to be attended to is to drain the plates slightly after coming from the silver-bath, and, if necessary, to remove the liquid from the back with blotting-paper, so as to introduce as little silver as possible into the nitrate of magnesia.   A solution of one grain of silver to the ounce is quite sufficient to keep the plates sensitive; and when the strength rises, as it will in time, to above a certain limit, the slight evaporation that always takes place will render the silver solution sufficiently strong to dissolve off

the iodide in small holes.   If this occur the bath can be restored by nearly, but not quite, precipitating the silver with a solution of chloride of magnesium, and then filtering.

One of the most important things to be attended to is the necessity of preserving the plates where they are perfectly free from any light.   It will be evident to all, that anything short of absolute darkness, when the sensitive surface is exposed to its action for day after day, and perhaps week after week, must be fatal to its subsequent cleanliness.   The necessity for protecting the plates from any deleterious gases, ammonia, for instance, is too obvious to require comment.

XVI. *Description of two Letter-scales.*
By Prof. Gerling *of Marburg*[*].

[With a Plate.]

THE introduction of postage-stamps renders it desirable that private individuals should possess the means of weighing letters with accuracy and facility.   It will perhaps be permitted me, in addition to the letter-scales already in use, to describe two others which a year's practice has proved correct, and which on account of their simplicity are comparatively cheap.

A side view of the first instrument is given in Plate I. fig. 1. It consists of the unmoveable slab of wood A, and the four small wooden pillars B, which latter are united by the thin steel axes C. The moveable portions consist, in the first place, of four equal bent levers D, C, E of sheet metal.   Through the ends of these levers pass the four axes DD and EE.   These axes carry the two strips of sheet metal DD, a piece of metallic gauze being spread across from one strip to the other to form the table on which the letter is placed.   Below are two similar strips of metal, EE, which serve as a counter-weight to the letter placed above.   The strips EE which remain horizontal, move more or less to the right according as the weight placed above is greater or less. The wire *d* is a fixed index, and the divisions which answer to a quarter of an ounce, half an ounce, an ounce, and so forth, are first determined empirically and marked along the strip EE.

The second balance, fig. 2, consists simply of a glass tube filled with water, into which another smaller tube passes and sinks to a certain depth; the latter tube is furnished with a little platform on the top, on which the letter is weighed.   The internal tube sinks when a weight is placed above, and the depth to which it sinks for certain weights is first empirically determined; these depths are marked upon the surface of the tube, and are the indices by which the weight of the letter placed above is given.

* Communicated by the Author.

XVII. *On the Law of Prime Numbers.*
By Charles James Hargreave, *Esq., LL.D., F.R.S.* [*]

IN a paper written by me in the year 1849, and published in the Philosophical Magazine, vol. xxxv. p. 36, I attempted, by means of certain principles there laid down, to apply the processes of analytical investigation to the theory of numbers; and I thereby arrived at certain conclusions relative to the occurrence of prime numbers in the ordinal series which were expressed in the following proposition :—" The average distance between two successive prime numbers at the point x in the ordinal series is $\log x$; and the average number of primes which may be expected to occur between x and x' is the logarithm-integral of x between those limits, or $\mathrm{li}x' - \mathrm{li}x$."

The nature and exact purport of this theorem will be readily understood by any person who takes the trouble of counting the number of primes between various limits, when he will discover that, by taking ranges of sufficient magnitude, the *rate* at which the primes occur appears to follow a very regular and uniform law, though nothing can be apparently more irregular than the particular places at which the individual primes are to be found.

In the paper above alluded to, the law was verified for various numbers under one million; and it was found that the formula always produced the proper number of primes within very moderate limits of error.

I propose in the present paper, first, to make a further investigation as to the exact nature of the formula, and of the demonstration upon which it rests; and secondly, to point out a practicable method by which the number of primes can be counted to a point in the ordinal series far beyond the limits of the existing tables of primes, a method which will of course enable us to apply a more satisfactory test to the formula itself.

Discussion of the Formula $\mathrm{li}x$.

For the process by which this formula was orginally obtained I must refer the reader to the paper above mentioned. It will suffice for the present purpose to state, that the power of analytically investigating such a question was made to depend upon the substitution of an analytical equivalent for a quantity which was necessarily from the nature of the problem indeterminate, but susceptible only of certain specified values. The equivalent thus substituted was, as may be anticipated, the arithmetical mean of the possible values.

Since the publication of that paper, my attention has been directed by my esteemed friend Mr. Sylvester to an investigation

of a similar problem, by Professor Tchebycheff of the University of St. Petersburgh, which appeared in the Transactions of the Academy of Sciences of St. Petersburgh, having been read before that learned body in the month of May, 1848*. In that memoir the same formula, li$x$, was arrived at by a process with which the paper in the Philosophical Magazine has nothing in common; and the formula was considered as denoting the limiting value of the number of primes up to x, as x increases without limit, rather than as being a mode of determining the average number of primes between given limits.

The results given in that paper, (to which, as being a novel and highly ingenious mode of applying analysis to a question in the theory of numbers, the attention of the reader is particularly called), may be thus stated. The author first demonstrates that if ϕx be used to denote the exact number of primes between 0 and x, then the expression

$$\Sigma\left\{\left(\phi(x+1)-\phi x-\frac{1}{\log x}\right)\frac{(\log x)^n}{x^{1+\rho}}\right\}$$

(the summation with respect to x extending from the commencement of the ordinal series 2 to $x = \infty$) approximates to a finite limit as ρ approximates to zero; from which, it is readily inferred that there are an infinite number of values of x for which ϕx lies between li$x \pm \dfrac{\alpha x}{(\log x)^n}$, however small α be taken, and however large n be taken.

The author then shows that any function of x which differs from li$x$ by a quantity of the order of magnitude denoted by $x \div (\log x)^n$, is incapable of approximating to ϕx within quantities of the order of $x \div (\log x)^n$; so that, for example, the conjectural formula given by Legendre would ultimately give results at variance from the truth by quantities of the order denoted by $x \div (\log x)^2$; though it will not begin permanently to deviate from the truth until x has reached a magnitude of about a million and a quarter.

The ultimate result is, that li$x$ expresses ϕx as accurately as it can be expressed by means of x and its logarithmic and exponential functions; and the expression approximates to truth as x increases without limit.

The following new investigation of the formulæ relating to the occurrence of prime numbers has been suggested, partly by a perusal of M. Tchebycheff's memoir, and partly by the theo-

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* "Sur la fonction qui détermine la totalité des Nombres Premiers inférieurs à une limite donnée," par M. le Prof. Tchebycheff. Lu le 24 Mai, 1848.

rems given in the latter part of my former paper. In that paper the term average is applied in its ordinary sense, which is well understood as meaning the rate at which primes occur at and about any particular points of the ordinal series.

It is easy, however, to suggest other and more algebraical meanings of the term average as applied to this subject. Let p_n denote the nth actual prime number; and let us compare the expression $\Sigma\phi(p_n)$ from $p_n=2$ to $p_n=\infty$ with another expression $\Sigma\phi(P_n)$ between the same limits, where P_n represents one of a series of terms $P_1, P_2, P_3 \ldots$ which are connected with each other by the law $P_{n+1}=P_n+\psi(P_n)$. Then if $\Sigma\phi(P_n)$ be equal to $\Sigma\phi(p_n)$, or differs therefrom only by quantities of an order lower than $\Sigma\phi(p_n)$, we may say that the two series $p_1, p_2, p_3 \ldots P_1, P_2, P_3 \ldots$ run *pari passu* with each other, though there may be no such thing as an absolute equality between any term of one series and the corresponding term of the other. The law of formation, which is strictly true with regard to the latter series, may be regarded as possessing a species of average truth with reference to the former series.

Lemma. The expression $\lambda\rho-\dfrac{1}{\rho}$, where

$$\lambda\rho=1+\frac{1}{2^{1+\rho}}+\frac{1}{3^{1+\rho}}+\frac{1}{4^{1+\rho}}+\ldots\ ad\ infinitum$$

approximates to γ, or $\cdot57712\ldots$ as ρ diminishes without limit. For

$$\int_0^\infty t^\rho\frac{\epsilon^{-t}}{1-\epsilon^{-t}}\,dt=\int_0^\infty t^\rho(\epsilon^{-t}+\epsilon^{-2t}+\epsilon^{-3t}+\ldots)dt=\Gamma(\rho+1)\lambda\rho.$$

But, since

$$\frac{1}{1-\epsilon^{-t}}=\frac{1}{t}+\frac{1}{2}+\frac{1}{12}t-\frac{1}{30}\frac{t^3}{\lceil 4}+\frac{1}{42}\frac{t^5}{\lceil 6}-\ldots,$$

we have

$$\int_0^\infty t^\rho\frac{\epsilon^{-t}}{1-\epsilon^{-t}}\,dt=\Gamma\rho+\frac{1}{2}\Gamma(\rho+1)+\frac{1}{12}\Gamma(\rho+2)-\frac{1}{720}\Gamma(\rho+3)+\ldots;$$

whence

$$\lambda\rho=\frac{1}{\rho}+\frac{1}{2}+\frac{1}{12}(\rho+1)-\frac{1}{720}(\rho+1)(\rho+2)(\rho+3)+\ldots,$$

and

$$\lambda\rho-\frac{1}{\rho}=\frac{1}{2}+\frac{1}{12}(\rho+1)-\ldots=\gamma\ (\text{when }\rho=0).$$

From this it follows, that as ρ diminishes, the logarithm of $\lambda\rho$ approximates to $-\log\rho+\gamma\rho$.

Now, by referring to my former paper, it will be seen that

$$\log(\lambda\rho) = -\log\left(1 - \frac{1}{2^{1+\rho}}\right) - \log\left(1 - \frac{1}{3^{1+\rho}}\right) - \log\left(1 - \frac{1}{5^{1+\rho}}\right) - \dots$$

$$= \frac{1}{2^{1+\rho}} + \frac{1}{3^{1+\rho}} + \frac{1}{5^{1+\rho}} + \frac{1}{7^{1+\rho}} + \dots$$

+ certain quantities which have a finite limit.

We may therefore conclude that the series

$$\frac{1}{2^{1+\rho}} + \frac{1}{3^{1+\rho}} + \frac{1}{5^{1+\rho}} + \frac{1}{7^{1+\rho}} + \dots$$

differs from the logarithm of ρ^{-1} by a quantity, which, as ρ diminishes without limit, approximates to a certain finite constant.

It will also be readily perceived, by integrating the function $\lambda\rho$ with respect to ρ, that the expression

$$\frac{1}{(\log 2)2^{1+\rho}} + \frac{1}{(\log 3)3^{1+\rho}} + \frac{1}{(\log 4)4^{1+\rho}} + \dots, \text{ or } -\int\lambda\rho\, d\rho + \rho,$$

approximates to $-\log\rho + (1-\gamma)\rho +$ a constant : from which we infer that

$$\Sigma \frac{1}{(\log n)n^{1+\rho}},$$

where n has every value from 2 to ∞, differs only by a constant from $\Sigma \dfrac{1}{\mu^{1+\rho}}$, where μ has the value of every *prime* number from 2 upwards, and ρ is supposed to be as small as we please; so that, when $\rho = 0$, these two expressions, which are infinite and of the order $-\log\rho$, differ only by a finite constant. Or

$$\Sigma \frac{1}{\mu} = C + \Sigma \frac{1}{n \log n}.$$

But $\Sigma \dfrac{1}{n \log n}$ is in reality the sum of the reciprocals of a series of numbers, each of which is equal to the one before it increased by its logarithm (see Phil. Mag. vol. xxxv. p. 49) ; and we thus see that the sum of the reciprocals of the prime numbers differs only by a constant from the sum of the reciprocals of a series of numbers which follow the law $P_{n+1} = P_n + \log P_n$, the magnitude of the constant being dependent on the point at which the latter series is made to commence. If we call the series which obeys the law $P_n + \log P_n = P_{n+1}$ the series of theoretical primes, and assign a proper value to the first term of the series, we may then assert that the infinite sum of the reciprocals of the real primes is equal to the infinite sum of the reciprocals of the theoretical primes; and we may, in the sense before indicated, assert that the average distance between two primes at the point x in the ordinal series is $\log x$.

A practical Method of ascertaining the exact number of Primes up to an advanced point in the Ordinal Series.

In the paper above referred to, I verified the formula li$x$ by computing it for various values of x up to a million; and compared the results with those derived from actually counting the primes up to that point from Burckhardt's and Chernac's tables. The number of primes up to a million, as counted, appeared to be 78,493, the formula giving 78,626. Since that period, I ascertained, by counting, the number of primes between two millions and three millions to be 67,751, the formula giving 67,916.

I now propose to point out a method of ascertaining the number of primes up to a point considerably beyond the limits of any tables; and to apply it to the number 10,000,000 and inferior numbers.

Let us denote by Px the exact number of primes inferior to x, and by $N\left(\dfrac{x}{p}\right)$ the number $\dfrac{x}{p}$ when integer, and the next whole number below $\dfrac{x}{p}$ when it is not integer. If we take at random a set of prime numbers $p_1, p_2, p_3 \ldots p_n$, the number of numbers between 0 and x which are not divisible by any of the set $p_1, p_2, \ldots p_n$, is

$$x - \Sigma\left\{N\left(\frac{x}{p}\right)\right\} + \Sigma\left\{N\left(\frac{x}{pp}\right)\right\} - \Sigma\left\{N\left(\frac{x}{ppp}\right)\right\} + \ldots$$
$$\pm N\left(\frac{x}{p_1 p_2 \ldots p_n}\right);$$

the Σ being intended to cover every combination of one, of two, of three, &c., as the case may be, taken out of the set $p_1, p_2 \ldots p_n$. For example, the number of numbers up to 10,000 not containing either 2, 3, 5, 7 or 11, will be

10,000
$- (5000 + 3333 + 2000 + 1428 + 909)$
$+ (1666 + 1000 + 714 + 454 + 666 + 476 + 303 + 285 + 181 + 129)$
$- (333 + 238 + 151 + 142 + 90 + 64 + 95 + 60 + 43 + 25)$
$+ (47 + 30 + 21 + 12 + 8)$
$- 4.$

This proposition is obvious from the considerations developed at pages 38 and 39 of the former paper, and its truth will immediately suggest itself on consideration*.

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* The theorem may be generalized as follows. Denote by A$_n$ the sum

The proposition is, however, useless when the number of primes to be eliminated is considerable; for the number being n, the number of terms to be found by actual division would be 2^{n+1}, if it were not that some of them vanish. It may be observed, however, that when the number x is large, the result will not differ materially from

$$x\left(1-\frac{1}{p_1}\right)\left(1-\frac{1}{p_2}\right)\ldots\left(1-\frac{1}{p_n}\right),$$

which can be formed at once.

If the number be of the form A $(p_1 \times p_2 \ldots \times p_n)$, the result is simply $A(p_1-1)(p_2-1)\ldots(p_n-1)$. Thus the number of numbers up to $2.3.5.7.11.13.17.19$, or 9,699,690, not divisible by either 2, 3, 5, 7, 11, 13, 17 or 19, is simply

$$2 \times 4 \times 6 \times 10 \times 12 \times 16 \times 18, \text{ or } 1,658,880.$$

It should be observed, that the result is exclusive of the primes p_1, p_2, &c. themselves, but inclusive of 1. It is scarcely necessary to say that the process is of the easiest possible character, though tedious from the number of divisions to be effected. I have ascertained by it that the number of ordinals up to ten millions not divisible by either 2, 3, 5, 7, 11, 13, 17, 19 or 23, is 1,635,877; and the corresponding result up to five millions is 817,944. If we suppose this process performed for all the primes up to the square root of x, the result would lead us at once to the accurate value of Px; but, for the reason above stated, the process is not practically applicable except for the purpose of

of the integer quotients obtained by dividing a given number A_0 by every possible combination of a set of primes, m in number, taken n and n together; and let the series

$$A_0 - A_1 t + A_2 t^2 - A_3 t^3 + \ldots \pm A_m \; t^m$$

be called ϕt.

Then $\phi(1)$ will express the number of ordinals not exceeding A_0 which do not contain any one of the m primes. Similarly, $-\phi'(1)$ will express the number of ordinals which contain one, and no more, of the set of m primes; $\frac{1}{2}\phi''(1)$ the number of ordinals which contain two, and no more, of these primes; and generally $\pm \frac{1}{1.2.3.p} \phi^{(p)}(1)$ will express the number of ordinals, each of which contains p distinct individuals of the set of primes, and no more.

Thus up to 10,000, the number of terms not containing 2, 3, 5, 7 or 11, is 2077; the number containing only one of this set is 4193; the number containing two distinct members of this set, and no more, is 2819; the number containing exactly three is 809; the number containing exactly four is 98; and the number of terms containing all the five is 4.

eliminating those numbers which contain the first 10 or 11, or at the utmost 12 primes. I therefore pass to another process.

If we take any number, say 10,000, and divide it successively by the prime numbers less than its square root, and if we count the number of primes between each divisor and its quotient (both included when prime), the aggregate of these results is obviously the number of numbers up to 10,000 which are composite of two primes, or double numbers. For each prime from p_1 to $\dfrac{x}{p_1}$ has a corresponding double number, of which p_1 is one factor and the prime in question is the other; so that this process exactly exhausts all the double numbers including squares.

If now we take each quotient obtained in the last process, down to that quotient which was obtained by dividing by the prime next below the cube root of x, and deal with it in exactly the same manner as we before dealt with the number itself, that is, divide it by every prime up to its square root, and count the primes from the divisor to the quotient, the aggregate of the results will exactly exhaust all the composites of three primes or *treble* numbers including cubes. The repetition of the operation upon each of the quotients in the last part of the process, or rather upon such of them as admit of the operation, gives us all the quadruple numbers; and so on, as far as the magnitude of the number enables us to carry the process.

The number of prime divisors diminishes as we proceed, but the number of dividends to be operated upon increases rapidly; so much so, that this process is utterly impracticable for those small prime divisors which occur early in the series, such as 2, 3, 5, &c.

We have, therefore, two processes; the first of which enables us to expel that large mass of composite numbers which contain the small primes, such as from 2 to 19 or 23, or if need be, to 29 or even 31, but is scarcely practicable beyond this point; and the second of which processes enables us to ascertain the number of composite numbers which include only the larger primes, such as those lying between 19 or 23 and the square root of the number, but which would be quite unavailing for the determination of the number of terms involving the small primes 2, 3, 5, &c. By using both processes, however, we bring the problem within the province of reasonable industry.

As the second process may not be very clear without an example, I will apply the two processes to the determination of the number of primes under one million, for which we shall not have occasion to use any table of primes beyond 35,000.

Using the first process for the primes from 2 to 23, we find

$$x = 1{,}000{,}000 \qquad \Sigma N\!\left(\frac{x}{p}\right) = 1{,}498{,}954$$

$$\Sigma N\!\left(\frac{x}{pp}\right) = 901{,}506 \qquad \Sigma N\!\left(\frac{x}{ppp}\right) = 286{,}846$$

$$\Sigma N\!\left(\frac{x}{pppp}\right) = 53{,}597 \qquad \Sigma N\!\left(\frac{x}{ppppp}\right) = 6{,}106$$

$$\Sigma N\!\left(\frac{x}{pppppp}\right) = 395 \qquad \Sigma N\!\left(\frac{x}{ppppppp}\right) = 6$$

$$\overline{1{,}955{,}498} \qquad\qquad \overline{1{,}791{,}912}$$

Thus learning 163,586 as the number of numbers whose lowest prime factor exceeds 23.

Using the second process for the primes above 23, we write down the following table in two columns; the first contains the quotients obtained by dividing x by the primes from 997 (the next prime below $x^{\frac{1}{2}}$) to 29, and the second contains the number of primes between the divisor and the quotient. The first column is obtained from a table of reciprocals, and the second column from Barlow's table of primes, where they are in effect already counted.

		66+		155+		289+		551+		1729+	
1003	1	1270	2	1686	1	2518	2	4484	11	16393	154
1009	3	1293	8	1703	5	2570	10	4739	42	16949	210
1017	5	1300	10	1733	10	2610	15	5025	78	18867	404
1023	8	1314	14	1751	13	2638	19	5076	82	21276	646
1029	9	1321	17	1757	15	2680	26	5181	96	23255	851
1034	12	1331	19	1776	17	2724	36	5235	103	24390	966
1049	15	1345	20	1795	22	2785	44	5524	138	27027	1223
1055	17	1353	21	1828	26	2832	51	5586	146	32258	1721
1062	19	1364	23	1848	29	2865	58	5780	168	34482	1944
1067	21	1375	26	1912	39	2881	60	5988	194		
1076	23	1390	28	1919	41	2967	71	6134	212		23680
1088	25	1410	31	1964	46	3021	78	6369	243		29603
1097	29	1426	33	1988	50	3154	92	6622	269		12284
1102	30	1447	39	2004	55	3194	99	6711	281		6530
1127	35	1464	43	2036	60	3215	102	7194	335		3325
1132	37	1477	45	2053	63	3257	109	7299	347		1006
1135	38	1485	48	2087	69	3412	129	7633	386		
1140	39	1512	54	2141	78	3533	145	7874	413		76428
1158	42	1517	55	2159	81	3558	150	8849	523		
1164	44	1531	58	2169	83	3610	157	9174	558		
1166	45	1545	60	2188	85	3690	168	9345	579		
1172	47	1555	63	2227	90	3717	173	9708	620		
1191	50	1560	65	2257	95	3802	184	9900	645		
1206	53	1584	70	2277	99	3891	196	10309	690		
1209	54	1615	76	2309	105	3984	207	11235	784		
1215	56	1620	78	2320	107	4149	229	12048	871		
1218	58	1631	81	2375	115	4184	234	12658	941		
1233	62	1647	83	2386	119	4291	250	13698	1049		
1236	63	1663	86	2444	128	4366	258	14084	1090		
1254	66	1669	89	2493	134	4405	262	14925	1178		
	1006		3325		6530		12284		29603		

Of the above quotients, the last sixteen only are greater than the squares of their divisors. We have therefore to divide each of these by the primes commencing with the divisor, and going on to the prime next below the square root of the quotient. The primes, counted as before from divisor to quotient, give the result of this part of the process as being 8664. These divisions produce only three instances in which the quotient exceeds the square of the divisor, and the repetition of the process upon these gives 8. The result with reference to 1,000,000 is—

No. of terms whose lowest prime factor exceeds 23		163,586
Of which we find double numbers		76,428
... triple numbers		8,664
... quadruple numbers		8
		85,100
		78,486

Adding the 9 excluded primes and deducting unity, we have the number of primes up to a million 78,494, being one more than Legendre counted.

The application of the formula to 10,000,000 is much more laborious. The results for 5 and 10 millions are as follows:—
For 5,000,000—

No. of terms whose lowest factor exceeds 23 is		817,944
Of which we find double numbers		395,600
... triple numbers		72,900
... quadruple numbers . . .		925
Leaving a final residue of primes		348,527
The number obtained by formula li$x$ is . .		348,634
The number given by Legendre's formula is		348,644

For 10,000,000 the results are—

No. of terms whose lowest factor exceeds 23 is		1,635,877
No. of double numbers included in these .		796,759
No. of triple numbers included		170,827
No. of quadruple numbers included . . .		3,667.

Leaving a final residue of primes, (excluding unity, but including the primes from 2 to 23), to the number of . . 664,632
The number obtained by the formula li$x$ is . . . 664,916
The number obtained by Legendre's formula is . . 665,140.

These results sufficiently attest the truth of the formula which I have previously demonstrated. Some years ago I obtained the following results by the above process, but the calculations were not made with so much care as those given in the present paper.

No. of primes up to 9,699,690		645,544
No. of primes up to 4,849,845		338,919

the corresponding results of the formula li$x$ being respectively 646,266 and 338,898.

Dublin, July 1, 1854.

XVIII. *On the Constitution of Quinine.* By ADOLPH STRECKER[*].

NOTWITHSTANDING the great importance of quinine, it is much less perfectly known to chemists than a multitude of compounds which possess no interest in a practical point of view. In fact, chemists are agreed neither as to its elementary composition, its equivalent, nor its molecule; and we are still further from being acquainted with the constitution of this alkaloid. It is clear, therefore, that before we shall be able to *compose* quinine, these preliminary questions must be resolved; and I think that by publishing my researches, I may perhaps assist those chemists who wish to see the solution of the problem of the artificial production of quinine.

The elementary composition of quinine is expressed at present by the formulæ $C^{19} H^{11} NO^2$ (Laurent) or $C^{20} H^{12} NO^2$ (Liebig). This is a question which may readily be settled by the analysis of pure quinine. I prepared this base in a state of purity from commercial sulphate of quinine, which I obtained through M. Merck in an extremely pure state, and purified still further by repeated crystallizations. The analysis of quinine, dried at 248° F., was effected with oxide of copper and oxygen gas; it gave me the formula $C^{20} H^{12} NO^2$, or in per-centage proportions,—

		Found.		Calculated.
Carbon	. . .	74·0	74·1	74·1
Hydrogen	. .	7·5	7·5	7·4
Nitrogen	. . .			8·6
Oxygen	. . .			9·9

This formula is also confirmed by the analysis of several of its compounds.

Nitrate of Quinine.—Sulphate of quinine precipitated by nitrate of baryta and left to spontaneous evaporation furnishes colourless rhombic crystals. The analysis of this compound, when purified by several crystallizations, led to the formula $C^{40} H^{24} N^2 O^4 . HO . NO^5$, or in a hundred parts,—

		Found.	Calculated.
Carbon	. . . .	62·1	62·0
Hydrogen	. . .	6·6	6·5

Sulphate of Quinine.—The sulphate from which I prepared the quinine, after drying at 248° F., furnished 10·7 and 10·8 per cent. of sulphuric acid, numbers which are almost identical with those calculated from the formula

$C^{40} H^{24} N^2 O^4 . HO . SO^3$ (calculation 10·7 per cent. SO^3).
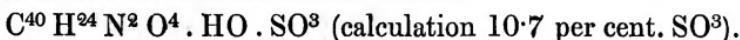
An alcoholic solution of quinine furnishes with nitrate of

silver a crystalline precipitate which dissolves in boiling water. When this solution is allowed to cool, it forms a pasty mass, which in course of time becomes converted into colourless crystals. These crystals are a compound of quinine and nitrate of silver; they are soluble only in 300 parts of cold water. Analysis leads to the formula $C^{40} H^{24} N^2 O^4 . AgO . NO^5$.

As regards the equivalent of quinine, some chemists represent it by the formula $C^{20} H^{12} NO^2$, whilst others think it is rather $C^{40} H^{24} N^2 O^4$. According to the first formula, neutral sulphate of quinine is $C^{20} H^{12} NO^2 HO . SO^3$; and the other sulphate, $C^{40} H^{24} N^2 O^4 HO . SO^3$, must be a basic sulphate. According to the second formula, it is the latter salt that is neutral, and the sulphate $C^{20} H^{12} NO^2 HO . SO^3$ is an acid salt which ought to be represented by the formula $C^{40} H^{24} N^2 O^4 2HO . 2SO^3$.

There are other chemists, again, who represent the equivalent of quinine by $C^{20} H^{12} NO^2$, but regard the molecule of this alkaloid as composed of two equivalents, or as $C^{40} H^{24} N^2 O^4$.

From the facts hitherto known, there appeared to be no very conclusive reasons for adopting one of these formulæ in preference to the other, but I think that the following results will enable us to settle the equivalent and molecule of quinine.

A mixture of iodide of æthyle and quinine dissolved in æther and left for a few hours, furnishes crystals which increase in quantity with time. These crystals dissolve with facility in boiling water, and separate again on cooling, in the form of long acicular crystals arranged round a common centre. They are colourless, silky, and of a bitter taste; they lose no water at 212° F., and fuse at a higher temperature without decomposition. The analysis of these crystals showed a composition expressed by the formula $C^{44} H^{29} N^2 O^4 I$, or in 100 parts,—

	Found.		Calculated.
Carbon . .	55·0	54·8	55·0
Hydrogen .	6·2	6·2	6·0
Iodine . .	26·4		26·5

The formation of this compound, which I name iodide of æthylo-quinine, is explained by the following equation:—

$$C^{40} H^{24} N^2 O^4 + C^4 H^5 I = C^{44} H^{29} N^2 O^4 I.$$

An analogous compound is obtained with iodide of methyle, which I call iodide of methylo-quinine; its composition is expressed by the formula $C^{42} H^{27} N^2 O^4 I$; its analysis gave—

	Found.	Calculated.
Carbon . . .	54·2	54·1
Hydrogen . .	5·9	5·8
Iodine . . .	26·9	27·2

The watery solutions of these iodides are not precipitated by ammonia, and only become turbid with a great excess of potash. The precipitate thus obtained dissolves in boiling water, and the solution gives crystals identical with the first; in fact, the iodide is not decomposed by the potash, but it is insoluble in a solution of that alkali. With oxide of silver, the solutions of these iodides furnish iodide of silver, and the solution retains a very energetic base; it rapidly absorbs carbonic acid from the air, and furnishes therewith crystals possessing an alkaline reaction. This base, to which I give the name of æthylo-quinine, may be obtained by evaporating the solution *in vacuo*, when it forms an amorphous mass; it dissolves in alcohol, and is precipitated from this solution by æther in colourless crystals. It is decomposed even at a temperature of 348° F. With this base I have prepared the following salts:—

Neutral sulphate of æthylo-quinine $\left.\right\}$ $C^{44} H^{29} N^2 O^5 . SO^3$

Acid sulphate of æthylo-quinine $\quad C^{44} H^{29} N^2 O^5 . SO^3 + HO . SO^3$.

Chloride of æthylo-quinine $\quad . \quad C^{44} H^{29} N^2 O^4 . Cl$.

Platinum double salt $\quad . \quad . \quad C^{44} H^{29} N^2 O^4 . Cl + HCl + 2 Pt Cl^2$.

All the properties of these compounds show that æthylo-quinine belongs to the fourth class of Hofmann's bases; it is a base corresponding with oxide of ammonium, NH^4O. Quinine is therefore a nitride base (of the third class), and contains in the molecule $C^{40} H^{24} N^2 O^4$ three compound radicals. There is no doubt after this that the molecule and equivalent of quinine must be expressed by the formula $C^{40} H^{24} N^2 O^4$. As to the compound radicals which enter into the composition of quinine, I should lose sight of the region of facts if I attempted to pronounce an opinion upon them.

XIX. *On some Extensions of Quaternions.* By Sir WILLIAM ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the French Institute, Hon. or Corr. Member of several other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[7.] LET us now consider generally the *associative* law of multiplication, which may be expressed by the formula already mentioned but reserved in [1.],

$$\iota . \iota' \iota'' = \iota \iota' . \iota''; \quad . \quad . \quad . \quad . \quad . \quad (51)$$

or by this other equation,

$$\iota_e . \iota_f \iota_g = \iota_e \iota_f . \iota_g : \quad . \quad . \quad . \quad . \quad (52)$$

and let us inquire into the conditions under which this law shall be fulfilled, for any 3 unequal or equal symbols of the form ι.

If the conception of the polynomial expression

$$P = \Sigma \iota x = \iota_0 x_0 + \iota_1 x_1 + \ldots + \iota_n x_n, \quad (1)$$

be no further restricted than it was in [1.], then *each* of the three indices e, f, g, in the equation (52), may receive any one of the $n+1$ values from 0 to n; so that there are in this case $(n+1)^3$ associative conditions of this form (52), whereof *each*, by comparison of the coefficients of the $n+1$ symbols ι, breaks itself up into $n+1$ separate equations, of the ordinary algebraical kind, making in all no fewer than $(n+1)^4$ algebraical relations, to be satisfied, if possible, by the $(n+1)^3$ constants of multiplication, of the form (fgh): respecting which constants, it will be remembered that the general formula has been established,

$$\iota_f \iota_g = (fg0)\iota_0 + \ldots + (fgh)\iota_h + \ldots + (fgn)\iota_n. \quad (7)$$

We may therefore substitute, in (52), the expressions,

$$\left.\begin{array}{l} \iota_f \iota_g = \Sigma_h(fgh)\iota_h, \; \iota_e \iota_f = \Sigma_h(efh)\iota_h, \\ \iota_e \iota_h = \Sigma_k(ehk)\iota_k, \; \iota_h \iota_g = \Sigma_k(hgk)\iota_k; \end{array}\right\} \quad . \quad . \quad (53)$$

and then, by comparing coefficients of ι_k, this associative formula (52) breaks itself up, as was just now remarked, into $(n+1)^4$ equations between the $(n+1)^3$ constants, which are all included in the following*:

$$\Sigma_h(fgh)(ehk) = \Sigma_h(efh)(hgk); \quad . \quad . \quad . \quad (54)$$

where the *four* indices $efgk$ may each separately receive any one of the $n+1$ values from 0 to n, and the summations relatively to h are performed between the same limits.

[8.] Introducing next the simplification (10) of article [2.], or supposing $\iota_0 = 1$, which has been seen to reduce the number of the constants of multiplication from $(n+1)^3$ to $(n+1)n^2$, we find that the number of the equations to be satisfied by them is reduced in a still greater ratio, namely from $(n+1)^4$ to $(n+1)n^3$. For, if we suppose the index g to become 0, and observe that each of the constants $(f0h)$ and $(0fh)$ is equal, by (12) and (13), to 0 or to 1, according as h is unequal or equal to f, we shall see that

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* This formula (54) may be deduced from the equation (214) in p. 239 of the writer's "Researches respecting Quaternions," published in the Transactions of the Royal Irish Academy, vol. xxi. part 2, by changing there the letters $r \, s \, t \, r' \, s'$ to $f \, h \, g \, e \, k$, and substituting the symbol (fgh) for $n_{g,f,h}$. Or the same formula (54) may be derived from one given in page (30) of the Preface to the same author's Lectures on Quaternions, (Dublin, Hodges and Smith, 1853), by writing $g f e k$ instead of $f g g' h'$, and changing each of the two symbols $1_{g,f,h}, 1'_{g,f,h}$, to (fgh). But the *general reductions* of the present paper have not been hitherto published.

the sum in the left-hand member of the formula (54) reduces itself to the term (efk): but such is also in this case the value of the right-hand sum in the same formula, because in calculating that sum we need attend only to the value $h = k$, if g be still $= 0$. In like manner, if $f = 0$, each sum reduces itself to (egk); and if $e = 0$, the two sums become each $= (fgk)$. If then any one of these three indices, e, f, g, be $= 0$, the formula (54) is satisfied: which might indeed have been foreseen, by observing that, in each of these three cases, one factor of each member of the equation (52) becomes $= 1$. We may therefore henceforth suppose that each of the three indices, e, f, g, varies only from 1 to n, or that

$$e > 0, \quad f > 0, \quad g > 0; \quad . \quad . \quad . \quad . \quad (55)$$

while k may still receive any value from 0 to n, and h still varies in the summations between these latter limits: and thus the number of equations, supplied by the formula (54), between the constants (fgh), is reduced, as was lately stated, to $(n+1)n^3$; while the number of those constants themselves had been seen to be reduced to $(n+1)n^2$, by the same supposition $\iota_0 = 1$.

[9.] Additional reductions are obtained by introducing the law of conjugation (32), or by supposing K . $\iota_f \iota_g = \iota_g \iota_f$, with the consequences already deduced from that law or equation in [5.]. Using Σ' to denote a summation relatively to h from 1 to n, and taking separately the two cases where $k = 0$ and where $k > 0$, we have, for the first case, by (54),

$$\Sigma'(efh)(gh) = \Sigma'(fgh)(eh); \quad . \quad . \quad . \quad . \quad . \quad (56)$$

and for the second case,

$$(ef)(g0k) - (fg)(e0k) = \Sigma'\{(efh)(ghk) + (fgh)(ehk)\}. \quad (57)$$

No new conditions would be obtained by interchanging e and g; but if we cyclically change efg to fge, each of the two sums (56) is seen to be equal to another of the same form; and two new equations are obtained from (57), by adding which thereto we find,

$$0 = \Sigma'\{(efh)(ghk) + (fgh)(ehk) + (geh)(fhk)\}; \quad . \quad (58)$$

and therefore,

$$(fg)(e0k) - (ef)(g0k) = \Sigma'(geh)(fhk). \quad . \quad . \quad . \quad (59)$$

When $e = f$, the equations (56) and (59) become, respectively,

$$0 = \Sigma'(fh)(fgh), \quad . \quad . \quad . \quad . \quad . \quad . \quad (60)$$

and

$$(fg)(f0k) - (f)(g0k) = \Sigma'(gfh)(fhk); \quad . \quad (61)$$

which are identically satisfied, if we suppose also $f = g$; the properties [5.] of the symbols (fgh) being throughout attended to: while, by the earlier properties [2.], the symbol $(e0k)$ or $(0ek)$ is

equal to 0 or to 1, according as e and k are unequal or equal to each other. And no equations distinct from these are obtained by supposing $e=g$, or $f=g$, in (56) and (59). The associative conditions for which $k=0$ are, therefore, in number, $n(n-1)$ of the form (60), and $\frac{1}{3}n(n-1)(n-2)$ of the form (56); or $\frac{1}{3}(n^3-n)$ in all. And the other associative conditions, for which $k>0$, are, in number, $n^2(n-1)$ of the form (61), and $\frac{1}{2}n^2(n-1)(n-2)$ of the form (59), or $\frac{1}{2}(n^4-n^3)$ in all. It will, however, be found that this last number admits of being diminished by $\frac{1}{2}(n^2-n)$, namely by one for each of the symbols of the form (fg); and that if, before or after this reduction, the associative equations for which $k>0$ be satisfied, then those other $\frac{1}{3}(n^3-n)$ conditions lately mentioned, for which $k=0$, are satisfied also, as a necessary consequence. The *total number* of the *equations of association*, included in the formula (54), will thus come to be reduced to

$$\tfrac{1}{2}(n^4-n^3)-\tfrac{1}{2}(n^2-n), \text{ or to } \tfrac{1}{2}n(n-1)(n^2-1);$$

but it may seem unlikely that even so large a number of conditions as this can be satisfied *generally*, by the $\frac{1}{2}n(n^2+1)$ constants of multiplication [5.]. Yet I have found, not only for the case $n=2$, in which we have thus 5 constants and 3 equations, but also for the cases $n=3$ and $n=4$, for the former of which we have 15 constants and 24 equations, while for the latter we have 34 constants and 90 equations, that all these associative conditions can be satisfied: and even in such a manner as to leave some degree of indetermination in the results, or some constants of multiplication disposable.

[10.] Without expressly introducing the symbols (fgh), results essentially equivalent to the foregoing may be deduced in the following way, with the help of the characteristics [3.] of operation, S, V, K. The formula of association (51) may first be written thus[*]:

$$\iota S\iota'\iota'' + \iota V\iota'\iota'' = S\iota\iota' . \iota'' + V\iota\iota' . \iota''; \quad . \quad . \quad . \quad (62)$$

in which the symbols $S\iota\iota'$ and $V\iota\iota'$ are used to denote concisely, without a point interposed, the scalar and vector parts of the product $\iota\iota'$, but a point is inserted, after those symbols, and before ι'', in the second member, as a mark of multiplication: so that, in this abridged notation, $S\iota\iota' . \iota''$ and $V\iota\iota' . \iota''$ denote the products which might be more fully expressed as $(S . \iota\iota') \times \iota''$ and $(V . \iota\iota') \times \iota''$; while it has been thought unnecessary to write any point in the first member, where the factor ι occurs at the left hand. Operating on (62) by S and V, we find the two following equations of association, which are respectively of the scalar and

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[*] There is here a slight departure from the notation of the Lectures on Quaternions, by the suppression of certain points, which circumstance in the present connexion cannot produce ambiguity.

vector kinds :

$$S(\iota V\iota'\iota'' - \iota''V\iota\iota') = 0 ; \quad . \quad . \quad . \quad . \quad . \quad (63)$$

$$V(\iota V\iota'\iota'' + \iota''V\iota\iota') = \iota''S\iota\iota' - \iota S\iota'\iota'' ; \quad . \quad . \quad (64)$$

because the law (32) of conjugation, $\iota'\iota = K\iota\iota'$, gives, by (41),

$$S\varpi'\varpi = + S\varpi\varpi', \quad V\varpi'\varpi = -V\varpi\varpi'.$$

For the same reason, no essential change is made in either of the two equations, (63), (64), by interchanging ι and ι''; but if we cyclically permute the three vector-units, $\iota \iota' \iota''$, then (63) gives

$$S(\iota V\iota'\iota'') = S(\iota'V\iota''\iota) = S(\iota''V\iota\iota') ; \quad . \quad . \quad . \quad (65)$$

and there arise three equations of the form (64), which give, by addition,

$$V(\iota V\iota'\iota'' + \iota'V\iota''\iota + \iota''V\iota\iota') = 0 ; \quad . \quad . \quad . \quad (66)$$

and therefore conduct to three other equations, of the form*

$$V(\iota V\iota'\iota'') = \iota''S\iota\iota' - \iota'S\iota''\iota. \quad . \quad . \quad . \quad . \quad (67)$$

Equating ι'' to ι, the two equations (65) reduce themselves to the single equation,

$$S(\iota V\iota\iota') = 0 ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (68)$$

and the formula (67) becomes

$$V(\iota V\iota\iota') = \iota^2\iota' - \iota S\iota\iota' : \quad . \quad . \quad . \quad . \quad . \quad (69)$$

both which results become identities, when we further equate ι' to ι. And no equations of condition, distinct from these, are obtained by supposing $\iota'' = \iota'$, or $\iota' = \iota$, in (65) and (67). The number of the symbols ι being still supposed $= n$, and therefore by [5.] the number of the constants which enter into the expressions of their n^2 binary products (including squares) being $= \frac{1}{2}(n^3 + n)$, these constants are thus (if possible) to be made to satisfy $\frac{1}{3}(n^3 - n)$ associative and scalar equations of condition, obtained through (63), from the comparison of the scalar parts of the two ternary products, $\iota \cdot \iota'\iota''$ and $\iota\iota' \cdot \iota''$; namely, $n(n-1)$ scalar equations of the form (68), and $\frac{1}{3}n(n-1)(n-2)$ such equations, of the forms (65). And the same constants of multiplication must also (if the associative law is to be fulfilled) be so chosen as to satisfy $\frac{1}{2}(n^3 - n^2)$ vector equations, equivalent each to n scalar equations, or in all to $\frac{1}{2}(n^4 - n^3)$ scalar conditions, obtained through (64) from the comparison of the vector parts of the same two ternary products (51); namely, $n(n-1)$ vector equations of the form (69), and $\frac{1}{2}n(n-1)(n-2)$ other vector equations, included in the formula (64). This new analysis therefore confirms completely the conclusion of the foregoing paragraph respecting the general existence of $\frac{1}{2}(n^4 - n^3)$

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* This formula is one continually required in calculating with quaternions (compare page li of the Contents, prefixed to the author's Lectures).

$+ \frac{1}{3}(n^3 - n)$ associative and scalar equations of condition, between the $\frac{1}{2}(n^3 + n)$ disposable constants of multiplication, when the general conception of the polynomial expression P of [1.] is modified by the suppositions, $\iota_0 = 1$ in [2.], and $\iota'\iota = K\iota\iota'$ in [5.]. At least the analysis of the present paragraph [10.] confirms what has been lately proved in [9.], that the number of the conditions of association can be *reduced so far*; but the same analysis will also admit of being soon applied, so as to assist in proving the existence of those *additional* and *general* reductions which have been lately mentioned without proof, and which depress the number of conditions to be satisfied to $\frac{1}{2}(n^4 - n^3) - \frac{1}{2}(n^2 - n)$. Meanwhile it may be useful to exemplify briefly the foregoing general reasonings for the cases $n = 2, n = 3$, that is, for trinomial and quadrinomial polynomes.

[11.] For the case $n = 2$, the two distinct symbols of the form ι may be denoted simply by ι and ι'; and the equations of association to be satisfied are all included in these two,

$$\iota \cdot \iota\iota' = \iota^2\iota', \quad \iota' \cdot \iota'\iota = \iota'^2\iota; \quad \cdot \quad \cdot \quad \cdot \quad (70)$$

which give, when we operate on them by S and V, two scalar equations of the form (68), and two vector equations of the form (69), equivalent on the whole to six scalar equations of condition, between the five constants of multiplication, (1) (2) (12) (121) (122), if we write, on the plan of preceding articles,

$$\iota^2 = (1), \quad \iota'^2 = (2), \quad S\iota\iota' = (12), \left.\begin{array}{c}\\\\\end{array}\right\} \quad \cdot \quad (71)$$
$$V\iota\iota' = -V\iota'\iota = (121)\iota + (122)\iota'.$$

From (68), or from (60), or in so easy a case by more direct and less general considerations, we find that the comparison of the scalar parts of the products (70) conducts to the two equations,

$$0 = (121)(1) + (122)(12) = (122)(2) + (121)(12). \quad \cdot \quad (72)$$

From (69), or (61), we find that the comparison of the vector parts of the same products (70) gives immediately four scalar equations, which however are seen to reduce themselves to the three following:

$$(121)(122) = -(12); \quad (122)^2 = (1); \quad (121)^2 = (2); \quad \cdot \quad (73)$$

the first of these occurring twice. And it is clear that the equations (72) are satisfied, as soon as we assign to (1) (2) and (12) the values given by (73). If then we write, for conciseness,

$$(121) = a, \quad (122) = b, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (74)$$

we shall have, for the present case ($n = 2$), the values,

$$(1) = b^2, \quad (2) = a^2, \quad (12) = -ab. \quad \cdot \quad \cdot \quad (75)$$

And hence (writing κ instead of ι'), we see that the *trinome*[*],

$$P = z + \iota x + \kappa y, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (76)$$

where xyz are ordinary variables, will possess all the properties of those *polynomial expressions* which have been hitherto considered in this paper, and especially the associative property, if we establish the formula of multiplication,

$$(\iota x + \kappa y)(\iota x' + \kappa y') = (bx - ay)(bx' - ay') \\ + (a\iota + b\kappa)(xy' - yx'); \quad \cdot \quad \cdot \quad \cdot \quad (77)$$

wherein a and b are any two constants of the ordinary and algebraical kind. In this trinomial system,

$$z'' + \iota x'' + \kappa y'' = (z + \iota x + \kappa y)(z' + \iota x' + \kappa y'), \quad \cdot \quad (78)$$

if

$$\begin{array}{l} x'' = zx' + z'x + a(xy' - yx'), \\ y'' = zy' + z'y + b(xy' - yx'), \\ z'' = zz' + (bx - ay)(bx' - ay'); \end{array} \right\} \quad \cdot \quad \cdot \quad \cdot \quad (79)$$

we have therefore the two *modular relations*,

$$\begin{array}{l} z'' + bx'' - ay'' = (z + bx - ay)(z' + bx' - ay'), \\ z'' - bx'' + ay'' = (z - bx + ay)(z' - bx' + ay'); \end{array} \right\} \quad \cdot \quad (80)$$

that is to say, the functions $z \pm (bx - ay)$ are *two linear moduli* of the system. A general theory with which this result is connected will be mentioned a little further on. Geometrical interpretations (of no great interest) might easily be proposed, but they would not suit the plan of this communication.

[12.] For the case $n = 3$, or for the *quadrinome*

$$P = x_0 + \iota_1 x_1 + \iota_2 x_2 + \iota_3 x_3, \quad \cdot \quad \cdot \quad \cdot \quad (81)$$

we may assume

$$\begin{array}{l} \iota_1{}^2 = a_1, \qquad \iota_2{}^2 = a_2, \qquad \iota_3{}^2 = a_3, \\ S\iota_2\iota_3 = b_1, \quad S\iota_3\iota_1 = b_2, \quad S\iota_1\iota_2 = b_3, \end{array} \right\} \quad \cdot \quad \cdot \quad \cdot \quad (82)$$

and

$$\begin{array}{l} V\iota_2\iota_3 = -V\iota_3\iota_2 = \iota_1 l_1 + \iota_2 m_3 + \iota_3 n_2, \\ V\iota_3\iota_1 = -V\iota_1\iota_3 = \iota_2 l_2 + \iota_3 m_1 + \iota_1 n_3, \\ V\iota_1\iota_2 = -V\iota_2\iota_1 = \iota_3 l_3 + \iota_1 m_2 + \iota_2 n_1; \end{array} \right\} \quad \cdot \quad \cdot \quad \cdot \quad (83)$$

and then the $\frac{1}{2}(n^4 - n^3) = 27$ scalar equations of condition, included in the vector form,

$$V(\iota \cdot \iota'\iota'') = V(\iota\iota' \cdot \iota''), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (84)$$

are found on trial to reduce[†] themselves to 24; which, after elimi-

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* I am not aware that this trinomial expression (76), with the formula of multiplication (77), coincides with any of the triplet-forms of Professor De Morgan, or of Messrs. John and Charles Graves : but it is given here merely by way of illustration.

† The reason of this reduction is exhibited by the general analysis in [14.].

nation of the 6 constants of the forms here denoted by a and b, or previously by (f) and (fg), furnish 18 equations of condition between the 9 other constants, of the forms here marked l, m, n, or previously (fgh); and these 18 equations may be thus arranged*:

$$0 = l_1(n_1-m_1) = l_2(n_2-m_2) = l_3(n_3-m_3),$$
$$0 = l_2(n_1-m_1) = l_3(n_2-m_2) = l_1(n_3-m_3), \quad \Big\} \quad . \quad . \quad . \quad (85)$$
$$0 = l_3(n_1-m_1) = l_1(n_2-m_2) = l_2(n_3-m_3);$$

$$0 = n_1^2 - m_1^2 \qquad = \qquad n_2^2 - m_2^2 \qquad = \qquad n_3^2 - m_3^2,$$
$$0 = (n_2+m_2)(n_1-m_1) = (n_3+m_3)(n_2-m_2) = (n_1+m_1)(n_3-m_3), \quad \Big\} \quad (86)$$
$$0 = (n_3+m_3)(n_1-m_1) = (n_1+m_1)(n_2-m_2) = (n_2+m_2)(n_3-m_3);$$

they are therefore *satisfied*, without any restriction on $l_1 l_2 l_3$, by our supposing

$$n_1 = m_1, \quad n_2 = m_2, \quad n_3 = m_3; \quad . \quad . \quad . \quad (87)$$

but if we do *not* adopt this supposition, they *require* us to admit this *other* system of equations,

$$0 = l_1 = l_2 = l_3 = n_1 + m_1 = n_2 + m_2 = n_3 + m_3. \quad . \quad . \quad (88)$$

Whichever of these two suppositions, (87), (88), we adopt, there results a corresponding system of values of the six recently eliminated constants, of the forms a and b, or (f) and (fg); and it is found† that these values satisfy, without any new supposition being required, the $\frac{1}{3}(n^3-n) = 8$ scalar equations, included in the general form

$$S(\iota . \iota'\iota'') = S(\iota\iota' . \iota''), \quad . \quad . \quad . \quad (89)$$

which are required for the associative property.

[13.] In this manner I have been led to the *two* following systems of *associative quadrinomials*, which may be called systems (A) and (B); both possessing all those general properties of the polynomial expression P, which have been considered in the present paper; and one of them including the quaternions.

For the system (A), the quadrinomial being still of the form (81), or of the following equivalent form,

$$Q = w + \iota x + \kappa y + \lambda z, \quad . \quad . \quad . \quad . \quad (90)$$

where $wxyz$ are what were called in [1.] the *constituents*, the

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* For it is found that each of the three constants $(eff)+(egg)$ must give a null product, when it is multiplied by any one of the constants $(e'f'g')$, or by any one of these other constants, $(e''f''f'')-(e''g''g'')$; if each of the three systems, efg, $e'f'g'$, $e''f''g''$, represent, in some order or other, but not necessarily in one common order, the system of the three unequal indices, 1, 2, 3.

† This fact of calculation is explained by the general analysis of [15.]. The values of a and b may be deduced from the formulæ, $a_1 = m_1^2 - l_2 l_3$, $b_1 = l_1 m_1 - m_2 n_3$, with others cyclically formed from these.

laws of the *vector-units* $\iota\kappa\lambda$ are all included in this formula of multiplication for *any two vectors,* such as

$$\rho = \iota x + \kappa y + \lambda z, \quad \rho' = \iota x' + \kappa y' + \lambda z' : \quad . \quad . \quad (91)$$

$$\begin{aligned}
(A) \, .. \, \rho\rho' = {} & (m_1^2 - l_2 l_3)xx' + (l_1 m_1 - m_2 m_3)(yz' + zy') \\
& + (m_2^2 - l_3 l_1)yy' + (l_2 m_2 - m_3 m_1)(zx' + xz') \\
& + (m_3^2 - l_1 l_2)zz' + (l_3 m_3 - m_1 m_2)(xy' + yx') \\
& + (\iota l_1 + \kappa m_3 + \lambda m_2)(yz' - zy') \\
& + (\kappa l_2 + \lambda m_1 + \iota m_3)(zx' - xz') \\
& + (\lambda l_3 + \iota m_2 + \kappa m_1)(xy' - yx') ; \quad . \quad . \quad . \quad (92)
\end{aligned}$$

and it is clear that *Quaternions*[*] are simply that particular *case* of such QUADRINOMES (A), for which the *six* arbitrary constants $l_1 .. m_3$ and the *three* vector-units $\iota\,\kappa\,\lambda$ receive the following values :

$$l_1 = l_2 = l_3 = 1, \quad m_1 = m_2 = m_3 = 0, \quad \iota = i, \quad \kappa = j, \quad \lambda = k. \quad . \quad (93)$$

For the other associative quadrinomial system (B), which we may call for distinction TETRADS, if we retain the expressions (90) (91), we must replace the formula of *vector-multiplication* (92) by one of the following form :

$$\text{(B)} \, .. \, \rho\rho' = (lx + my + nz)(lx' + my' + nz')$$
$$+ (\kappa n - \lambda m)(yz' - zy') + (\lambda l - \iota n)(zx' - xz') + (\iota m - \kappa l)(xy' - yx') ; (94)$$

involving thus only *three* arbitrary constants, *lmn*, besides the *three* vector-units, $\iota\,\kappa\,\lambda$; and apparently having no connexion with the *quaternions,* beyond the circumstance that one common analysis [12.] conducts to both the *quadrinomes* (A), and the *tetrads* (B).

As regards certain *modular properties* of these two quadrinomial systems, we shall shortly derive them as consequences of the general theory of polynomes of the form P, founded on the principles of the foregoing articles.

[14.] In general, the formula (59) gives, by [2.], the two following equations, which may in their turn replace it, and are, like it, derived from the comparison of the *vector parts* of the general associative formula, or from the supposition that $k > 0$ in (54) :

$$(fg) = \Sigma(geh)(fhe), \text{ if } e \gtrless g ; \quad . \quad . \quad . \quad (95)$$

$$0 = \Sigma(geh)(fhk), \text{ if } k \gtrless e, \, k \gtrless g ; \quad . \quad . \quad (96)$$

the summation extending in each from $h = 1$ to $h = n$. Interchanging f and g in (95), we have

$$(gf) = \Sigma(feh)(ghe), \text{ if } e \gtrless f ; \quad . \quad . \quad . \quad (97)$$

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[*] See the author's Lectures, or the Philosophical Magazine for July, 1844, in which the first printed account of the quaternions was given.

and making $g=f$, in either (95) or (97), we obtain the equation,

$$(f)=\Sigma(feh)(fhe), \text{ if } e \gtrless f. \quad . \quad . \quad . \quad . \quad (98)$$

For each of the n symbols (f), there are $n-1$ distinct expressions of this last form, obtained by assigning different values to e; and when these expressions are equated to each other, there result $n(n-2)$ equations between the symbols of the form (fgh). For each of the $\frac{1}{2}n(n-1)$ symbols of the form (fg), where f and g are unequal, there are $n-1$ expressions (95), and $n-1$ other expressions of the form (97), because, by (33) and (36), $(gf)=(fg)$; and thus it might seem that there should arise, by equating these $2n-2$ expressions for each symbol (fg), as many as $2n-3$ equations from each, or $\frac{1}{2}n(n-1)(2n-3)$ equations in all between the symbols (fgh). But if we observe that the sums of the $n-1$ expressions (95) for (fg), and of the $n-1$ expressions (97) for (gf), are, respectively,

$$\left.\begin{array}{l} (n-1)(fg)=\Sigma_e\Sigma_h(geh)(fhe), \\ (n-1)(gf)=\Sigma_e\Sigma_h(feh)(ghe); \end{array}\right\} \quad . \quad . \quad (99)$$

where the summations may all be extended from 1 to n, because (ffh) and (ggh) are each $=0$, by (35), since $h > 0$; and that these two double sums (99) are equal; we shall see that the formula

$$(gf)=(fg), \quad . \quad . \quad . \quad . \quad . \quad (100)$$

though true, gives no information respecting the symbols (fgh): or is not to be counted as a new and distinct equation, in combination with the $n-1$ equations (95), and the $n-1$ equations (97). In other words, the comparison of the sums (99) shows that we may confine ourselves to equating separately to each other, for each pair of unequal indices f and g, the $n-1$ expressions (95) for the symbol (fg), and the $n-1$ other expressions (97) for the symbol (gf), without proceeding afterwards to equate an expression of the one set to an expression of the other set. We may therefore suppress, as unnecessary, an equation of the form (100), for each of the $\frac{1}{2}n(n-1)$ symbols of the form (fg), or for each pair of unequal indices f and g, as was stated by anticipation towards the close of paragraph [9.]. There remain, however, $2(n-2)$ equations of condition, between the symbols (fgh), derived from each of those $\frac{1}{2}n(n-1)$ pairs; or as many as $n(n-1)(n-2)$ equations in all, obtained in this manner from (95) and (97), regarded as separate formulæ. Thus, without yet having used the formula (96), we obtain, with the help of (98), by elimination of the symbols (f), (fg), (gf), through the comparison of $n-1$ expressions for each of those n^2 symbols, $n^2(n-2)$ equations of condition, homogeneous and of the second

dimension, between the symbols of the form (fgh). And without any such elimination, the formula (96) gives immediately $\frac{1}{2}n^2(n-1)(n-2)$ other equations of the same kind between the same set of symbols; because after choosing any pair of unequal indices e and g, we may combine this pair with any one of the n values of the index f, and with any one of the $n-2$ values of k, which are unequal both to e and to g. There are therefore, altogether, $\frac{1}{2}n^2(n+1)(n-2)$ homogeneous equations of the second dimension, obtained by comparison of the *vector parts* of the general formula of association, to be satisfied by the $\frac{1}{2}n^2(n-1)$ symbols of the form (fgh).

[15.] To prove now, generally, that when the *vector parts* of the associative formula are thus equal, the *scalar parts* of the same formula are necessarily equal also, or that the system of conditions (56) in [9.] is included in the system (57) or (59); we may conveniently employ the notations S and V, and pursue the analysis of paragraph [10.], so as to show that the system of equations (65), including (68), results from the system (67), including (69); or that if the formula (84) be satisfied for every set of three unequal or equal vector-units, $\iota \iota' \iota''$, then, for every such set, the formula (89) is satisfied also. For this purpose, I remark that the formula of *vector-association* (67), when combined with the *distributive* principle of multiplication [1.], and of operation with S and V [5.], gives generally, as in quaternions, the transformation

$$V\rho V\sigma\tau = \tau S\rho\sigma - \sigma S\rho\tau; \quad . \quad . \quad . \quad . \quad (101)$$

where ρ, σ, τ may denote *any three vectors*, and the symbol $V\rho V\sigma\tau$ is used to signify concisely the vector part of the product $\rho \times V(\sigma\tau)$; whence also we may derive by (41) this other general transformation,

$$V(V\sigma\tau . \rho) = \sigma S\rho\tau - \tau S\rho\sigma. \quad . \quad . \quad . \quad (102)$$

If then we write

$$V\sigma\tau = \rho', \quad V\tau\rho = \sigma', \quad V\rho\sigma = \tau', \quad . \quad . \quad (103)$$

and introduce another arbitrary vector ϖ, we shall have

$$V\rho'\varpi = \sigma S\tau\varpi - \tau S\sigma\varpi; \quad . \quad . \quad . \quad . \quad (104)$$

and therefore

$$V\rho V\rho'\varpi = \tau' S\tau\varpi + \sigma' S\sigma\varpi; \quad . \quad . \quad . \quad (105)$$

but also

$$V\rho V\rho'\varpi = \varpi S\rho\rho' - \rho' S\rho\varpi; \quad . \quad . \quad . \quad (106)$$

whence

$$\varpi S\rho\rho' = \rho' S\rho\varpi + \sigma' S\sigma\varpi + \tau' S\tau\varpi, \quad . \quad . \quad (107)$$

and consequently

$$S\rho\rho' = S\sigma\sigma' = S\tau\tau' : \quad . \quad . \quad . \quad . \quad . \quad (108)$$

but this is precisely by (103) the formula of *scalar-association*

(65), stated in its most general form. The general dependence of (65) on (67), or of (56) on (57), is therefore proved to exist; and the $\frac{1}{3}(n^3-n)$ associative conditions, for which $k=0$ in (54), are seen to be consequences of the $\frac{1}{2}(n^4-n^3)$ other conditions for which $k>0$; or even of those conditions diminished in number by $\frac{1}{2}(n^2-n)$, according to what was stated by anticipation in [9.], and has been proved by the analysis of [14.]. This result is the more satisfactory, because otherwise the *conditions of association* would essentially involve a system of homogeneous equations of the *third* dimension relatively to the symbols (fgh), obtained by substituting in (56) the expressions (95) or (97) for the symbols of the form (fg), including the values (98) of the symbols (f). But we see now (as above stated) that the *total* number of *distinct* conditions may be reduced to $\frac{1}{2}(n^4-n^3)$ $-\frac{1}{2}(n^2-n)$, between the total number $\frac{1}{2}(n^3+n)$ of constants of multiplication; or finally, after the *elimination* of the $\frac{1}{2}(n^2+n)$ symbols of the forms (f) and (fg), to a *system of homogeneous equations of the second dimension*, namely those determined in [14.], of which the number amounts (as in that paragraph) to

$$\tfrac{1}{2}(n^4-n^3)-n^2=\tfrac{1}{2}n^2(n+1)(n-2), \quad . \quad . \quad (109)$$

between the symbols of the form (fgh), whereof the number is

$$\tfrac{1}{2}(n^3+n)-\tfrac{1}{2}(n^2+n)=\tfrac{1}{2}n^2(n-1). \quad . \quad . \quad (110)$$

[16.] For example, when $n=2$, the *two* constants (121) and (122) have been seen in [11.] to be unrestricted by *any* condition. When $n=3$, we have 9 constants, lately denoted by $l_1\,l_2\,l_3\,m_1\,m_2\,m_3\,n_1\,n_2\,n_3$, wherewith to satisfy 18 homogeneous equations of the second dimension, namely those marked (85) and (86) in [12.]; which it has been seen to be possible to do, in two distinct ways (A) and (B), and even so as to leave some of the constants arbitrary, in each of the two resulting systems, of *associative quadrinomes and tetrads.* A similar result has been found by me to hold good for the case $n=4$, or for the case of *associative quines,* such as

$$\mathrm{P}=w+\iota x+\kappa y+\lambda z+\mu u, \quad . \quad . \quad . \quad (111)$$

involving *four* vector-units $\iota\,\kappa\,\lambda\,\mu$, which obey the laws of conjugation (32), and of association (51). For although there are in this case only $24=\frac{1}{2}n^2(n-1)$ constants of the form (fgh), to satisfy $80=\frac{1}{2}n^2(n+1)(n-2)$ homogeneous equations of the second dimension, yet I have found that the *forms** of these equations

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* The subject may be illustrated by the very simple remark, that although the four equations $tx=0$, $ty=0$, $ux=0$, $uy=0$, are such that *no three* of them *include* the *fourth*, since we might (for example) satisfy the three first alone by supposing $t=0$, $x=0$, yet they can *all four* be *satisfied* together by

are such as to allow this to be done in various ways, and even without entirely determining the constants. And it appears not impossible that similar results may be obtained for higher values of n; or that *associative**** polynomes of higher orders than *quines* may be discovered.

Observatory of Trinity College, Dublin,
July 4, 1854.

[To be continued.]

XX. *Mineralogical Notices.* *By* Dr. ALFRED DAY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following notices of British species and their localities, which have not as yet found their way into our standard works on mineralogy, may be interesting to a portion of your readers.

Ankerite.—For some years past I have noticed on the boards of the mineral dealers at Clifton ornamental specimens of iron ore, which have been sold as the produce of the neighbourhood, but which after considerable inquiry I find are brought from Whitehaven to the ports of South Wales. They consist of siliceous ironstone covered with hæmatite, then coated with specular iron, often of highly iridescent hues, with quartz almost dodecahedral, or in which the prism planes are much reduced, with a great variety of forms of carbonate of lime and what appears to be brown spar. The latter, which has the curvilinear faces common in this species, is sometimes dark brown and at others of a nankeen-yellow with a wax-like surface. On analysis I generally find that this last consists roughly of about one-half

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supposing either $x=0$, $y=0$, or $t=0$, $u=0$. Compare the equations (85) or (86), which are of the forms $tx=0$, $ty=0$, $tz=0$, $ux=0$, $uy=0$, $uz=0$, $vx=0$, $vy=0$, $vz=0$. In the theory of *quines,* however, the forms are not quite so simple.

* The *octaves,* or *octonomial expressions,* which Mr. Cayley published in the Philosophical Magazine for March 1845, and which had been previously but privately communicated to me by Mr. J. T. Graves about the end of 1843, after my communication to him of the quaternions, are *not associative polynomes.* Thus in Mr. Cayley's notation, the four following of his seven *types,* (123) (624) (176) (734), give $\iota_1 . \iota_2 \iota_4 = \iota_1 \iota_6 = -\iota_7$, but $\iota_1 \iota_2 . \iota_4 = \iota_3 \iota_4 = +\iota_7$; or with Mr. Graves's symbols, the triads ijk, ion, jln, klo, give $i.jl=in=-o$, but $ij.l=kl=+o$. See note to page (61) of the Preface to my Lectures. It was my perceiving this latter property of Mr. Graves's symbols in 1844, which chiefly discouraged me from pursuing the study of those *octaves,* as a species of *extension of the quaternions,* which Mr. Graves as well as Mr. Cayley had designed them to be, and which in one sense no doubt they are.

carbonate of lime, while the remaining portion is composed of the carbonates of iron, magnesia, and manganese in varying proportions. In various pieces which I have tried I have found upwards of 20 per cent. of carbonate of magnesia, and never, I believe, less than 10; while the reaction for manganese with soda on platinum-foil, though interfered with by the tendency of the mineral to grow dark under the blowpipe flame, is still very evident. I do not pretend to the last degree of accuracy in these examinations, and when the acknowledged difficulties of the quantitative determination of magnesia are considered, this will be excused. The analysis of carbonate of magnesia varies much in different hands, as may be seen by reference to Brande ; and I do not feel certain whether, in the combination with lime and iron, the water should be regarded as an essential constituent. On the principle of carbonate of magnesia entering as an isomorphic element into the union in question, I should think not. By a reference to the analyses of foreign ankerite, it will be noticed that while the carbonate of lime appears to be of nearly uniform amount, agreeing almost exactly with my determination, the quantities of carbonate of iron, magnesia and manganese, which enter as isomorphic elements, are quite capricious, and all my trials give results within the same limits. There cannot, therefore, be any doubt of the close agreement of the species. The quartz is sometimes singularly distorted by the unusual enlargement of some of its planes, so as to create a momentary difficulty in recognizing the form, and in other cases forms thin plates cut at all angles to the axis. In one instance it forms a thin hexagonal table, in which all the edges are bevelled, corresponding to the situation of the twelve pyramidal planes, and all the prism planes are reduced to thin lines of light when held so as to catch the reflexion.

Topaz.—Some time back a friend placed in my hands portions of the rocks of Lundy Island; and on looking at a small specimen composed of quartz crystals, I found three topazes among the group, which I recognized by their crystalline form. They can be traced quite through the body of the quartz crystals nearly three-fourths of an inch long, but so that the latter have interfered with their perfect development, indicating that they have penetrated while the material of each was soft. Since this another friend has brought from the island a detached crystal, which he picked up there, exhibiting a curious fracture where it has been torn from its matrix, which on examination with the lens exhibits numerous small crystalline prominences, apparently smaller individuals. This may, in fact, prove that it is not a fracture proper, but such a termination as is often seen in tourmalines, and which is thought to be connected with their electric

properties. The considerable size of these topazes renders them interesting as English specimens, those of Cornwall and the Mourne Mountains, Ireland, being generally very minute, I believe.

Silex.—From the table of a mineral dealer on Clifton Downs I lately obtained a portion of a large ironstone nodule, coated internally with quartz, which is concealed under an accumulation of siliceous matter, whose form is unknown to me, but possesses a characteristic structure, having much the appearance of anthophyllite in this respect, a kind of indistinct radiating texture, composed of extremely fine laminæ in groups, and of a pinkish hue. It is composed almost wholly of pure silex, with traces of iron and lime, and possibly alumina, which I have not attempted to separate from the iron, the former however so much in excess as to render it probable that their presence is either accidental or a mechanical admixture. The large nodule in which this is contained is said to have been brought from the summit of Broadfield Down, Somersetshire, where trap rocks make their appearance at no great distance. The quartz is so concealed by it, that it looks as though the former had been changed into it, or its crystallization suspended by some alteration of heat or pressure, or other cause, so that the elements have arranged themselves in a new form.

Celestine.—The beautiful and unique variety of this mineral, found at Pyle Hill, near Bristol, presenting fine crystals of the primitive rhombic prism with single replacements on the alternate solid angles, so well investigated and described by Mr. William Sanders, of Bristol, some years back, has not as yet, that I know of, been noticed in any of the manuals. Some of the smaller crystals, though retaining the general rhombic form, show minute replacements in addition to the principal one on the alternate solid angle, whose symbol it would be interesting to determine, and of which I have preserved specimens.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 74.]

May 18, 1854.—The Earl of Rosse, President, in the Chair.

THE following paper was read :—" On some conclusions derived from the observations of the Magnetic Declination at the Observatory of St. Helena. By Colonel Edward Sabine, R.A., V.P.R.S.

The author commences with the following preliminary remarks :—

" The part taken by the Royal Society in promoting, by its influence with Government, the establishment of the Colonial Magnetic Observatories, and in drawing up instructions for the guidance of those

who were employed in them, makes it the duty of the person charged with their superintendence, to spare no pains to place before the Fellows, on suitable occasions, the results of researches designed to obtain a foundation of facts, on which a correct theory of the magnetic variations might be framed, and an insight be gained into the nature of the physical agency by which they are produced.

" In this first stage of scientific inquiry, when we have only the phænomena themselves to guide us in their classification, or to indicate by apparent correspondences the existence of some causal connexion of which we have no other knowledge than that which the observations themselves may afford, the first difficulty to be met consists, in disentangling from the complication in which the magnetic variations proceeding from different causes first present themselves, the effects which may appear to be due to certain amongst them ; and in presenting these in some methodical order or arrangement, which may best assist the physicist or the mathematician in his conception of the problem or problems, to the solution of which he may desire to apply himself.

" The first and most obvious separation of the magnetic variations is into those which are presented at one time at different parts of the earth's surface, and have special reference therefore to space ; and those which present themselves at different times at one and the same place, and have special reference therefore to time. It is the object of magnetic surveys to collect the facts of the first, and of magnetic observatories the facts of the second, of these primary divisions. The present communication belongs to the second, and regards the variations depending upon time at a single station (St. Helena).

" Still, however, the phænomena even at a single station are too complicated for ready comprehension, and stand in need of further subdivision. This is most satisfactorily effected by the customary separation into three classes, or *elements* as they are frequently termed, the Declination, the Inclination, and the Intensity of the Directive Force. The discussion is limited on the present occasion to a single element, the Declination, and to a portion only of the results obtained by the observations of that element at St. Helena."

After premising a description of the instrument with which the observations were made, and of the mode of observing and of recording the observations, which is omitted here because it may be found in the Introduction to the first volume of the ' St. Helena Magnetical Observations,' the author proceeds to the conclusions which he desires to notice, and to the manner in which these have been obtained, which we follow, by adopting, as nearly as may be convenient, his own words.

" Before we attempt to examine those periodical variations, or fluctuations about a mean value, which, from their having for periods, for example, the solar year or the solar day, we naturally refer to causes depending in some way upon the earth's place in its orbit relatively to the sun, or to the earth's revolution round its axis, it is desirable to examine, and if practicable, to eliminate the effects of a variation which we have reason to believe belongs intrinsically to the

magnetism of the earth itself. The geographical aspect, if we may so express it, of the terrestrial magnetism, or the different measure in which the magnetic force exists at different parts of the earth's surface, and the different directions which a magnet assumes in different places by virtue of this force, so far from being permanent, are found to be subject to a continual change, which differs from all other magnetic variations with which we are acquainted, inasmuch as it does not present to us the character of an oscillation of the phænomena around a mean value in periods of greater or less duration, but appears, especially when viewed generally in its operation over the whole globe, as a continuously progressive change; it has for this reason received the appropriate name of '*secular change.*' It is possible indeed that the magnetism of the earth may have its periods,—that the phænomena existing at one and the same epoch over the whole surface of the globe may be identically reproduced at a subsequent epoch,—and that what has been called the secular change of each of the magnetic elements, which we perceive to be in progress at any particular point of the surface, St. Helena for example, may be part of a succession of changes which operate in a cycle, of which the duration, vast as it may be, may hereafter be found to be calculable. But as far as our knowledge has yet gone, it is insufficient to justify the assumption of even approximate periodical laws of this variation of the terrestrial magnetism; and we must continue to regard it therefore for the present as a secular change, of which the period, if there be one, or the periods, if there be more than one, are as yet unknown. But although the secular change has no intrinsic relation, as far as we have been able to discover, to any of the periods of time determined by other phænomena, either of our own planet or of any other of the heavenly bodies, it is obvious that we may assign the average rate at which the change is taking place, in any of the magnetic elements and at any particular station (the declination for example at St. Helena), corresponding to any definite measure of time in usage amongst us (say for example a month, or the twelfth part of a solar year), by taking the successive differences between the monthly means of all the hourly observations in the first and second months of their continuance, then between the second and third months, then between the third and fourth, and so on. By thus proceeding in the case of the Declination at St. Helena, we have sixty differences thus accruing in the five years of hourly observation, by which we find that the monthly increase of West Declination during these five years amounted on the average to $0'\cdot657$, or to an annual increase of $7'\cdot88$.

" It is not however necessary for this investigation that the system of observation should be *hourly* : a much less onerous system is sufficient, provided that the observations be distributed equably through the year, and that the intervals between the observations of each day be, approximately at least, equidistant. Before the commencement of the hourly series there had been fifteen months of two-hourly observations, and after its close the observations were continued for twenty-one months more at five hours of each day, the hours being such as to give by their combination a true mean value

for each day. We are thus enabled to take in a more extended period, amounting to ninety-six consecutive months, or eight years, from which to derive the average rate of secular change at St. Helena. Proceeding as before, we find for this period an average rate of $0'\cdot661$ for the increase of West Declination in a month, or an annual increase of $7'\cdot93$ in a solar year. During these eight years the horizontal magnetic direction at St. Helena had consequently changed altogether rather more than one degree.

" When the number of years are few from which an annual average rate of secular change is derived, it is necessary to be particular in regard to the regular distribution of the observations as to months and hours, because observations made at one time of the year or at one hour of the day, are not strictly comparable with those made at other times of the year or at other hours of the day, unless indeed corrections based on a long series of observations at the same spot or in its vicinity are applied for the annual and diurnal variations. But when the periods of comparison include intervals of considerable length, the comparative influence of the annual and diurnal variations is greatly diminished, and, if the comparison extend over a *great* number of years, it may practically be disregarded. Now, St. Helena being a naval station, and frequently visited by navigators of our own and other countries, who have had the requisite knowledge and have been at the pains to take the necessary precautions to make trustworthy observations, we are able to collect from the narratives of their voyages a succession of determinations of the Declination, all made at the same spot, namely, at the one anchorage at St. Helena, which extend over a period of 236 years, or from 1610 to 1846. The following Table contains eleven such determinations, all from authorities of high repute, which are fortunately so far equably distributed in respect to the years when they were made, as to throw light not only upon the average amount of the secular change of declination during that long period, but also in a considerable degree upon the regularity, or uniformity with which the change has taken place. By treating these eleven determinations according to well-known methods, we obtain $11°\ 48'$ as the west declination corresponding to the middle epoch, the year 1763, and $8'\cdot05$ as the most probable rate of the annual increase during the 236 years.

Declinations observed at the Anchorage at St. Helena.

1610. Davis	$-\ 7\ 13$	Calculated	$-\ 8\ 44$	Obs.—Calcul.	$+1\ 31$
1677. Halley	$-\ 0\ 40$	,,	$+\ 0\ 16$	,,	$-0\ 56$
1691. Halley	$+\ 1\ 00$	,,	$+\ 2\ 08$	,,	$-1\ 08$
1724. Mathews	$+\ 7\ 30$	,,	$+\ 6\ 34$	,,	$+0\ 56$
1775. Wales	$+12\ 18$	,,	$+13\ 25$	,,	$-1\ 07$
1789. Hunter	$+15\ 30$	,,	$+15\ 18$	,,	$+0\ 12$
1796. Macdonald	$+15\ 48$	,,	$+16\ 14$	,,	$-0\ 26$
1806. Krusenstern	$+17\ 18$	,,	$+17\ 34$	,,	$-0\ 16$
1839. Du Petit-Thouars	$+22\ 17$	,,	$+22\ 00$	,,	$+0\ 17$
1840. Ross	$+22\ 53$	,,	$+22\ 08$	,,	$+0\ 45$
1846. Bérard	$+23\ 11$	,,	$+22\ 57$	,,	$+0\ 14$

Mean Epoch 1763

Mean Declination $+11°\ 48'$

Annual Increase of West Declination $8'\cdot05$

" We have here then a striking example of the magnitude and character of the changes wrought at a particular station by this very remarkable feature of the earth's magnetic force. In less than two centuries and a half, the horizontal direction which a magnet takes at St. Helena by virtue of the terrestrial magnetic force has been found to have changed more than 30°, or more than a twelfth part of the whole circle : and when we further examine the facts more closely, we find reason to conclude that this great change has taken place by a steady, equable and uniform progression throughout the whole period. The rate of annual change derived from the eight years during which the observations were maintained by the detachment of the Royal Artillery stationed at the Observatory (7'·93) differs so slightly from that derived from the observations made at the anchorage from the earliest period at which observations are recorded (*i. e.* 8'·05), that we may practically regard them as the same. To examine whether this has been a uniform rate throughout the 236 years, or otherwise, the same calculation which gives 8'·05 as the most probable *average* rate of change between 1610 and 1846, will give also for each of the years in which the Declination was observed the most probable values of the Declination corresponding to the same rate of change supposed uniform. These calculated values are placed in the Table opposite to the years to which each belongs, and adjoining the observed values. The differences are shown in the next column. On inspecting these, we perceive that not one of the differences exceeds the limits, which, with a due consideration of the irregularities to which magnetic observations made on board ship are liable, may be ascribed to accidents of observation ; and, what is still more important, that they fall indiscriminately to the east and to the west of the values calculated on the supposition of a uniform rate, and without the slightest appearance of any systematic character which might indicate that the rate had been otherwise than regular. We have reason to conclude, therefore, that, from the earliest date to which we can refer, the progression of secular change at St. Helena has gone on from year to year, as nearly as may be, in one uniform *annual* rate.

" The instruction to be derived from the St. Helena observations does not however stop here. By a suitable arrangement of the observations of the eight years, they may be made to show that, when allowance is made for comparatively very small irregularities superimposed upon the regular march of the phænomenon by disturbing causes which will be treated of in the sequel, the average annual change takes place by *equal aliquot portions in each month of the year*. The eight years of observation commenced with June 1841 : if we take a mean of the eight monthly means in the eight Junes from 1841 to 1848, we shall have a better assured mean value of the Declination corresponding to the month of June, than if we had confined ourselves to a single year. If we then do the same with the eight Julys, and with each of the other months in succession, we shall have twelve monthly values for a year commencing with June and ending with May, which will represent in a simple

and condensed form the means of the whole eight years. These are exhibited in the next table, and we perceive at the first view that the increase of west declination is progressive in each month of the year without a single exception. If we desire to examine further the degree of approximation which these values present to a progression absolutely uniform, we may apply an aliquot portion of the annual value ($7'\cdot93$) to each of the monthly means corresponding to the difference in time from the mean epoch (December 1). These ali-

Months.	Mean Declination.	Correction for secular change to Dec. 1.	Mean Declination in the year.	Differences $(\psi - \psi')$.
June	23 23·42	+3·64	23 27·06 = ψ'	+0·22
July	23 24·45	+2·97	23 27·42 = ψ'	−0·14
August	23 24·91	+2·31	23 27·22 = ψ'	+0·06
September	23 25·30	+1·65	23 26·95 = ψ'	+0·33
October	23 26·32	+0·99	23 27·31 = ψ'	−0·03
November	23 27·07	+0·33	23 27·40 = ψ'	−0·12
December	23 27·73	−0·33	23 27·40 = ψ'	−0·12
January	23 28·29	−0·99	23 27·30 = ψ'	−0·02
February......................	23 29·23	−1·65	23 27·58 = ψ'	−0·30
March.........................	23 29·76	−2·31	23 27·45 = ψ'	−0·17
April	23 30·21	−2·97	23 27·24 = ψ'	+0·04
May...........................	23 30·69	−3·64	23 27·05 = ψ'	+0·23
Mean, corresponding to Dec. 1	23 27·28		23 27·28 = ψ	

quot portions are shown in the second column, and it will be seen by the third column, containing the mean declinations of the year deduced severally from the observation-values in the different months, with the correction for secular change assumed uniform applied, how very nearly the results derived from the several months approximate to one and the same value. The small differences which are shown in the last column are for the most part such as would probably disappear by a longer continuance of the observations; but we may notice, by the character of the signs, that there is also visible amongst them the indication of a comparatively very small semi-annual affection, depending on the sun's position on either side of the equator, which will be reverted to when treating of superimposed effects.

" The same features of regularity and uniformity are manifested if the examination be further pursued into shorter periods, by comparing with each other the twenty-six *fortnightly* means in the year; but enough has been already stated to show the magnitude, the regularity, and the systematic character of the changes called secular, which are thus produced by forces in constant operation at the surface of our planet. In our entire inability to connect these changes with any other of the phænomena of nature, either cosmical or terrestrial, we appear to have no other alternative than to view them as a constituent feature of the terrestrial magnetic force itself, and as one of its most remarkable characteristics, not to be overlooked by those who would seek to explain the phænomena of that force by

means of a physical theory. The attempts which have sometimes been made to explain them by a supposed connexion of the terrestrial magnetic phænomena with the distribution of land and sea at the surface of the globe, or with the distribution of heat on that surface, or by electrical currents excited by the rotation of the earth on its axis, contain no provision to meet a systematic variation of this nature; and break down altogether when the facts of the secular change are duly apprehended. From the phænomena of a single element at a single station, as here presented, we may assure ourselves that effects proceeding with so much order and regularity, which we cannot ascribe to any other cause than that of the terrestrial magnetism itself, and cannot therefore separate from its other manifestations, must find a place in any physical theory which professes to explain the phænomena of the earth's magnetism. To learn the changes in this and in the other magnetic elements which are simultaneously in progress in other parts of the globe, and to apprehend their mutual connexion and the general system of secular change which they indicate, it is necessary that the facts should be collected in the same manner as at St. Helena, at a great number of stations distributed over the earth's surface, and that they should be studied both separately and together. This may indeed appear a work of labour; but it is the most certain, if not the only certain mode of arriving at a correct knowledge of phænomenal laws, when the laws of their causation are wholly unknown. In this, as in similar studies, however complex the phænomena may appear at the first aspect,—and it is fully admitted that those of the secular magnetic change do appear extremely complex at the first view,—the mind soon begins to recognize order amidst apparent irregularity, and system amidst incessant variation. The order and regularity with which we are impressed at a single station are soon perceived to characterize, in an equally remarkable manner, a general systematic change taking place connectedly over the whole surface of the globe, and which can everywhere be traced to have been continuously in operation since the earliest epoch of magnetic observation. To those who find pleasure in tracing phænomena of great apparent complexity to laws of comparative simplicity which appear to embrace them all, this study affords its own repayment; and it is indispensable towards the acquisition of a knowledge of the laws of terrestrial magnetism. By a comparison of the isogonic lines corresponding to different epochs (lines of equal Magnetic Declination employed by Halley and since found so useful in generalisation in this branch of the magnetic phænomena), we perceive that a secular change of the Declination, almost identical with that at St. Helena, has prevailed at the same time over the greater part of the southern Atlantic; and that from the *form* of the isogonic lines in that quarter of the globe (which has undergone very little variation in the last 200 years), the regularity of the progression, and its persistence in the same direction, is in accordance with that general progressive motion from east to west, which magneticians have long since recognized as distinguishing the general systematic

change in the southern hemisphere from that in the northern, which takes place in the opposite direction; whilst from the form of the isogonic lines in that quarter, we may further anticipate that, at St. Helena, the secular change of the Declination will continue to take place in the same direction as at present, until the line drawn through the conical summits of the isogonic curves shall in its western progress pass the geographical meridian of that station."

The author then proceeds to the Variations which are found to take place in periods corresponding to a solar year and a solar day; a correspondence which, he remarks, " enables us to recognize a physical connexion, although we are still uncertain as to the mode of operation between cause and effect. A correct knowledge of the phænomena themselves is the surest guide to a correct judgement amongst the many theories which have been propounded in anticipation of that knowledge; and I have therefore taken this opportunity of bringing before the Society a careful analysis of the primary annual and diurnal variations at St. Helena attributable to solar influence, in the belief that they will be found to place in a very distinct light some points which are important to be kept in view in framing or in judging of such theories." For this purpose diagrams were exhibited, representing on a large scale the mean diurnal variation of the Declination at St. Helena in the different months of the year, and the annual variation at each of the twenty-four hours, both derived from the mean of five years of hourly observation; the secular change having been previously eliminated, these diagrams were regarded by the author as exhibiting what might be considered as typical views of the annual and diurnal variations, correct in their relations to the mean Declination in the year, or to the arithmetical mean of all the hourly observations in the year, taken as zero. As on the first aspect the diurnal phænomena in the several months are seen to separate themselves into two groups, having the equinoxes as at least approximate epochs of separation, the months in which the sun is north of the equator were coloured red, and those in which he is south of the equator were coloured blue.

Having in these diagrams the conjoint representation of two distinct classes of phænomena, a diurnal variation in each of the months, and an annual variation at each of the hours, the author proceeded to treat of each of these variations separately, commencing with the annual, which he illustrated by taking the hour of 7 A.M. as an example, and (referring to the diagram) showing the order and succession of the several months in the annual cycle at that hour, which are as follows:—in April the mean declination is about half a minute east of the mean declination in the year; in May about 2′ east; in June about $2\frac{1}{4}$ east; in July and August, when the sequence is slightly irregular, respectively 2′·1 and 2′·6 east; in September the declination is again approaching the mean line, being less than $1\frac{1}{2}$ east of it; in October it has passed the mean line, being about $1\frac{3}{4}$ *west* of it; November, December, January and February are congregated near the western extremity of the annual range, whilst in March we perceive that the declination is again approach-

ing the mean line, and in April it has passed to the east of the mean line. "We have here, then," the author proceeds, "in the successive changes of the declination in the course of the year, the general fact of the existence of an annual variation, of which, at the solar hour of 7 A.M., selected as an example, or when the sun is five hours east of the meridian, the phænomena are such as have been thus cursorily described. Were there no annual variation at that hour the different months would all have the same mean declination, and the extended figure, which in the diagram represents the annual cycle, would be concentrated into one point. The annual variation differs considerably at the different hours; but it is a general feature amongst them that the months on either side of the one solstice are either congregated together towards one extremity of the annual range at the hour, whilst the months on either side of the opposite solstice are similarly congregated at the opposite extremity, or the months of both solstices are contemporaneously in pretty rapid transition from the one extremity to the other. It is this *annual* variation which has been overlooked in the supposition entertained by a very eminent authority, that in the vicinity of the equator the magnetic direction would be found to be constant at all hours of the day and night. If we group together the monthly means of each period of six months separated by the equinoxes, we have two semiannual mean lines, each differing comparatively very slightly from any one of the months of which it is composed, but the two differing very greatly from each other, and both differing very considerably from the mean diurnal march in the year. If the latter line, viz. *the mean diurnal march in the year,* be projected as a straight line, as is done in the zero-line of fig. 1 in the annexed woodcut, the semiannual groups take respectively the forms exhibited in that figure, the continuous line being the semiannual march in the half year when the sun is north of the equator, and the dotted line the semiannual march when the sun is south of the equator. It is in this form that the phænomena of the annual variation in different parts of the globe may be most advantageously compared with each other. Fig. 2 represents the analogous phænomena at Toronto in 43° north, and fig. 3 those at Hobarton in 43° south latitude. The semiannual groups at Toronto and Hobarton have been obtained in precisely the same manner as those at St. Helena; the scale is the same in the three figures, *i. e.* ·5 of an inch to 1'·0 of Declination, the dotted and continuous lines refer respectively to the same periods of the year, and the zero line is in each figure the mean diurnal variation in the year at the station.

" In viewing these three figures, it is scarcely possible to doubt that they represent substantially the same phænomenon. The magnitude and inflexions of the curves are not indeed identical, but they approach so near to it that we may well suppose the small differences to be very minor modifications which will some day receive their explanation. It will be remarked that during the hours when the sun is above the horizon and the effects are greatest, the correspondence of the phænomena at the three stations is most striking,

Black line.—Mean Semiannual Diurnal Variation, March 22 to September 20.
Dotted line.—Mean Semiannual Diurnal Variation, September 22 to March 20.

Fig. 1.—St. Helena.
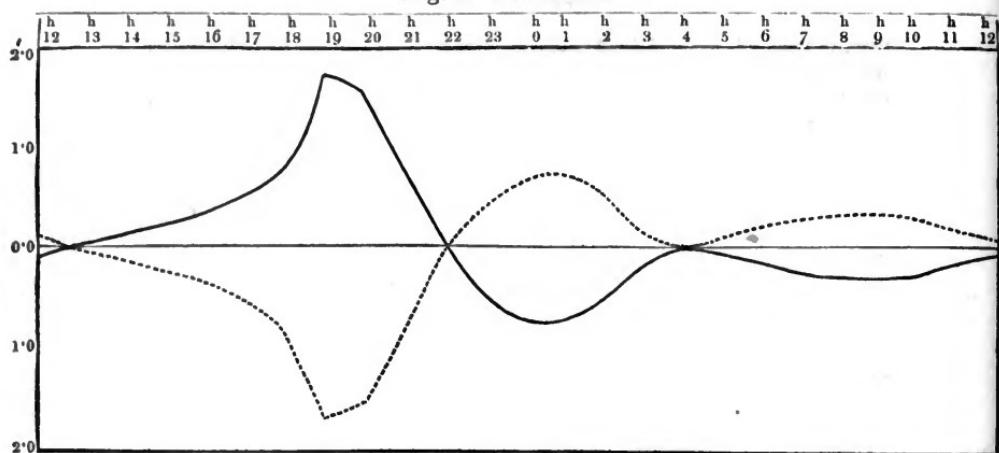
Fig. 2.—Toronto.
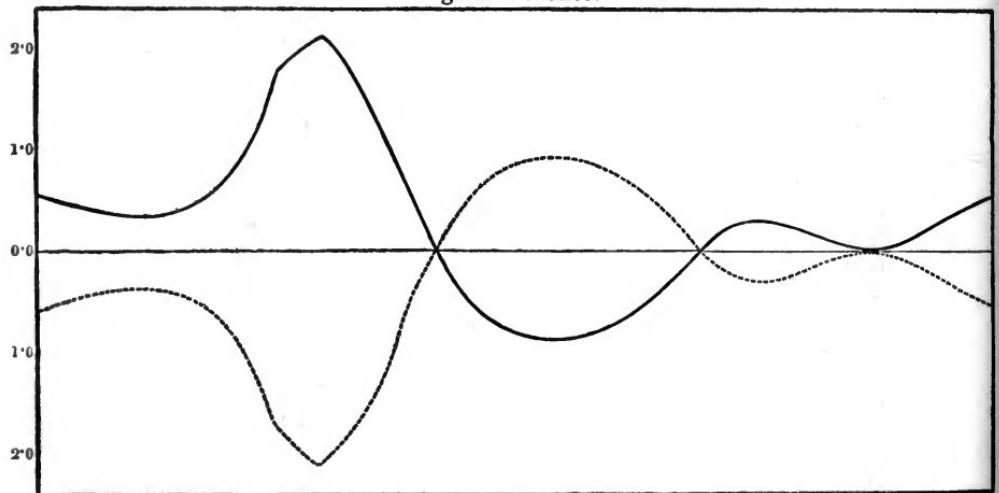
Fig. 3.—Hobarton.
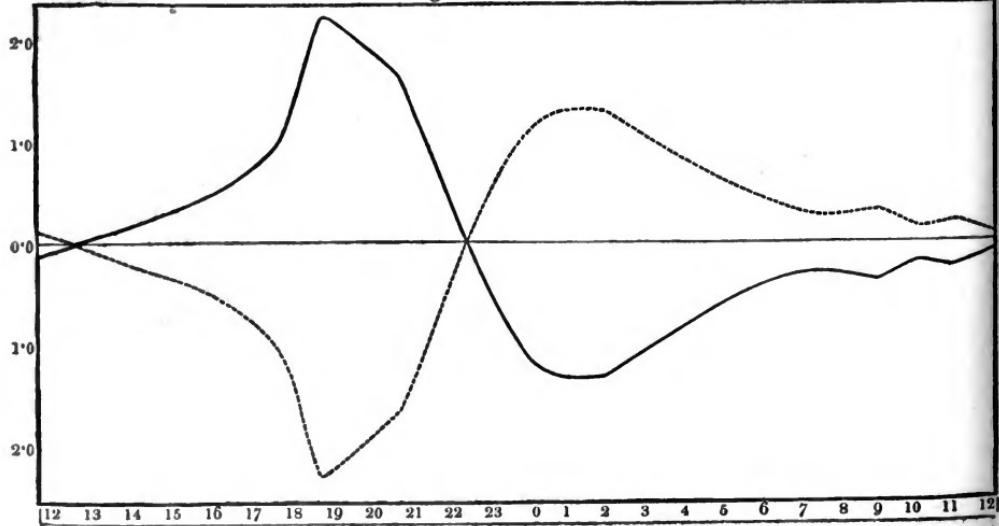
and that *there is no inversion of the phænomena in the opposite hemi-spheres*; in both (as well as at St. Helena, in the tropics), the De-clination is easterly of the mean in the forenoon and westerly in the afternoon when the sun is north of the equator, and the reverse when the sun is south of the equator. The effects are the same at the three stations, though in the one hemisphere the sun being north of the equator corresponds to summer, and in the other hemi-sphere to winter; whilst in the tropics this distinction of seasons almost ceases to be sensible, and the epochs of maximum and mini-mum of temperature do not correspond with either of those of the extra-tropical stations. The phænomena thus represented embrace above 86° of latitude, presenting not only almost extreme contem-poraneous diversities of climate, but also not less remarkable diver-sities of absolute dip, declination and magnetic force.

"No doubt can, I apprehend, be entertained that the annual variation which is here represented, is attributable, primarily, to the earth's revolution round the sun in a period of the same duration and in an orbit inclined to the equator. But in what way, it may be asked, does the sun superimpose upon the earth's magnetism this comparatively small but systematic magnetic variation? The simi-larity of effect, amounting almost indeed to identity at the hours when the sun is above the horizon of the station, taking place at stations where both the climatic and the terrestrial magnetic con-ditions are so dissimilar, seems to remove it altogether from those physical connexions, which have so often and in so many various ways been referred to as affording possible explanations of the mag-netic variations. In this difficulty some assistance may perhaps be afforded by examining more closely, by means of the St. Helena observations, the epochs when the phænomena of one of the semi-annual groups passes into the very dissimilar phænomena of the other semiannual group. This has been stated to take place *approximately* at the equinoxes. The approximation, particularly at the September equinox, is very distinctly and definitely marked. The day of the equinox is the 21st of September; if a mean be taken of the diurnal march in the three weeks from the 1st to the 21st of September, the line which represents it scarcely differs sensibly at any hour of the twenty-four from the mean line of the pre-ceding half-year, taken from the 22nd of March to the 20th of Sep-tember; thus showing that the phænomena of that semiannual group are unchanged up to the time of the equinox. If in the same way a mean be taken of the diurnal march in the three weeks following the 21st of September, the line which represents them shows that the passage from the phænomena of one semiannual group to those of the other has not only commenced, but that in half the period of three weeks, *i. e.* within eleven days of the equinox, the change has already advanced very far towards its completion; and by the middle of Oc-tober it is found to be quite complete, the mean in October retaining no trace of those semiannual characters which had undergone no modification ten days before the equinox." The facts thus stated were illustrated by diagrams.

" At the March equinox the commencement of the change is equally definite : no trace of change can be discovered in the mean from the 1st to the 20th of March, when compared with the mean of the six months from the 22nd of September to the 20th of March ; the change then commences, but from some cause not yet apparent, the conversion from the phænomena of the one half-year to those of the other is effected less rapidly at this than at the September equinox. The mean of the month of April retains the distinct traces of the group which it has quitted, and is in fact a month of transition between the two groups, but in May the conversion is quite complete ; the phænomena of that month have no characteristic distinguishable from those of June, July and August.

" From what has been stated in the preceding paragraphs, it will be evident that the epochs of the sun's passage of the equator have a very marked influence on the phænomena under consideration, and that the influence is the same and produces similar effects whether the station itself be north or south of the equator, and however diverse may be its climatic or magnetic conditions. The semiannual characteristics continue unchanged up to the days of the respective equinoxes ; these form the epochs when the transition from the characters of the one semiannual group to those of the other commences, the transition being completed a very few days after the September equinox, but somewhat less rapidly after the March equinox. Like the changes in the induced magnetism of ships, which follow immediately the changes in the terrestrial magnetism corresponding to the ship's altered geographical position, but *complete the change only after intervals of time of greater or less duration,* so the changes which we are here considering appear to commence at the equinoxial epochs, but to require a greater or less interval of time for their completion."

The divergence of the semiannual groups at the different hours from a mean march in the year has been shown in figs. 1, 2 and 3 by their comparison with the latter projected as straight lines, because the accordance of the divergence at the three stations is seen thereby in its simplest form. In another diagram the lines thus projected as straight lines were exhibited in their true Declination values, and compared with a Zero-line representing at each station the mean Declination in the year. " In the previous comparison of the *annual* variations at the three stations with each other, it was shown that there is no *inversion,* or *contrariety,* between the phænomena at Toronto and Hobàrton as representatives of opposite hemispheres, the same semiannual group diverging (during the hours of the day when the characters are most marked) in the same direction at the same hours at both stations. But markedly opposite characteristics are shown when we compare the divergences of the *mean diurnal* variation in the year from the zero-line at different stations ; these divergences, so far from according with each other at the two stations, present a strong contrast throughout ; the divergence at Toronto being to the east at the hours when at Hobarton it is to the west, and *vice versâ.* St. Helena, moreover, which agrees with both the other stations in the divergences of the semiannual groups, differs from both in those

of the mean of the whole year. The phænomena of the solar *annual* variation superimposed upon those of the solar *diurnal* variation,— and those of the solar *diurnal* variation itself,—are in this respect contradistinguished by important differences.

"To have completed the view of the solar variations of the Declination at St. Helena would have required a notice of the so-called irregular disturbances of that element, which are now known to have a periodical character dependent on solar hours; and also of the remarkable cycle which is found to pervade all the magnetic variations depending upon the sun, corresponding in its period and epochs with those of the phænomena of the solar spots; but as both these subjects have been recently brought before the Society in separate memoirs, the author does not think it necessary to do more than merely advert to them on the present occasion."

June 15.—The Earl of Rosse, President, in the Chair.

The following paper was read :—" On Osmotic Force." By Prof. Graham, V.P.R.S. (The Bakerian Lecture.)

This name was applied to the power by which liquids are impelled through moist membrane and other porous septa in experiments of endosmose and exosmose. It was shown that with a solution of salt on one side of the porous septum and pure water on the other side (the condition of the osmometer of Dutrochet when filled with a saline solution and immersed in water), the passage of the salt outward is entirely by diffusion, and that a thin membrane does not sensibly impede that molecular process. The movement is confined to the liquid salt particles, and does not influence the water holding them in solution, which is entirely passive : it requires no further explanation. The flow of water inwards, on the other hand, affects sensible masses of fluid, and is the only one of the movements which can be correctly described as a current. It is osmose, and the work of the osmotic force to be discussed.

As diffusion is always a double movement—while salt diffuses out, a certain quantity of water necessarily diffusing in at the same time in exchange—diffusibility might be imagined to be the osmotic force. But the water introduced into the osmometer in this way has always a definite relation to the quantity of salt which escapes, and can scarcely rise in any case above four or six times the weight of salt, while the water entering the osmometer often exceeds the salt leaving it, at least one hundred times. Diffusion is therefore quite insufficient to account for the water current.

The theory which refers osmose to capillarity appears to have no better foundation. The great inequality of ascension assumed among aqueous fluids is found not to exist, when their capillarity is correctly observed : and many of the saline solutions which give rise to the greatest osmose are undistinguishable in ascension from pure water itself.

Two series of experiments on osmose were described, the first series made with the use of porous mineral septa, and the second series with animal membrane. The earthenware osmometer con-

sisted of the porous cylinder employed in voltaic batteries, about 5 inches in depth, surmounted by an open glass tube 0·6 inch in diameter, attached to the mouth of the cylinder by means of a cap of gutta percha. In conducting an experiment the cylinder was filled with any saline solution to the base of the glass tube, and immediately placed in a large jar of distilled water; and as the fluid within the instrument rose in the tube, during the experiment, water was added to the jar so as to prevent inequality of hydrostatic pressure. The rise (or fall) of liquid in the tube was highly uniform, as observed from hour to hour, and the experiment was generally terminated in five hours. From experiments made on solutions of every variety of soluble substance, it appeared that the rise or osmose is quite insignificant with neutral organic substances in general, such as sugar, alcohol, urea, tannin, &c.; so also with neutral salts of the earths and ordinary metals, and with chlorides of sodium and potassium, nitrates of potash and soda and chloride of mercury. A more sensible but still very moderate osmose is exhibited by hydrochloric, nitric, acetic, sulphurous, citric and tartaric acids. These are surpassed by the stronger mineral acids, such as sulphuric and phosphoric acid and sulphate of potash, which are again exceeded by salts of potash and soda possessing either a decided acid or alkaline reaction, such as binoxalate of potash, phosphate of soda and carbonates of potash and soda. The highly osmotic substances were also found to act with most advantage in small proportions, producing in general the largest osmose in the proportion of one-quarter per cent. of salt dissolved. Osmose is eminently the phænomenon of weak solutions. The same substances are likewise always chemically active bodies, and possess affinities which enable them to act upon the material of the earthenware septum. Lime and alumina were accordingly always found in solution after osmose, and the corrosion of the septum appeared to be a necessary condition of the flow. Septa of other materials, such as pure carbonate of lime, gypsum, compressed charcoal and tanned sole-leather, although not deficient in porosity, gave no osmose, apparently because they are not acted upon chemically by the saline solutions. Capillarity alone was manifestly insufficient to produce the liquid movement, while the *vis motrix* appeared to be chemical action.

The electrical endosmose of Porrett, which has lately been defined with great clearness by Wiedemann, was believed to indicate the possession of a peculiar chemical constitution by water, while liquid, or at least the capacity to assume that constitution when polarized and acting chemically upon other substances. A large but variable number of atoms of water are associated together to form a liquid molecule of water, of which an individual atom of oxygen stands apart forming a negative or chlorous radical, while the whole remaining atoms together are constituted into a positive or basylous radical, which last will contain an unbalanced equivalent of hydrogen giving the molecule basicity, as in the great proportion of organic radicals. Now it is this voluminous basylous radical that travels in the electrical decomposition of pure water, and resolves itself into hydrogen

gas and water at the negative pole, causing the accumulation of water observed there, while the oxygen alone proceeds in the opposite direction to the positive pole. Attention was also called to the fact that acids, and alkalies, when in solution, are chemically combined with much water of hydration, sulphuric acid for instance evolving heat when the fiftieth equivalent of water is added to it. In the combination of such bodies, the disposal of the water is generally overlooked. Osmose was considered as depending upon such secondary results of combination, that is, upon the large number or voluminous proportions of the water molecules involved in such combinations. The porous septum is the means of bringing out and rendering visible, both in electrical and ordinary osmose, this liquid movement attending chemical combinations and decompositions.

Although the nature and *modus operandi* of chemical action producing osmose remains still very obscure, considerable light is thrown upon it in the application of septa of animal membrane. Ox-bladder was found to acquire greatly increased activity, and also to act with much greater regularity when first divested of its outer muscular coat. Cotton calico also impregnated with liquid albumen and afterwards exposed to heat so as to coagulate that substance, was sufficiently impervious, and formed an excellent septum, resembling membrane in every respect. The osmometer was of the usual bulb-form, but the membrane was supported by a plate of perforated zinc, and the instrument provided with a tube of considerable diameter. The diameter of the tube being one-tenth of that of the mouth of the bulb or the disc of membrane exposed to the fluids, a rise of liquid in the tube, amounting to 100 millimeters, indicated that as much water had permeated the membrane and entered the osmometer, as would cover the whole surface of the membrane to a depth of one millimeter, or one twenty-fifth part of an inch. Such millimeter divisions of the tube become degrees of osmose, which are of the same value in all instruments.

Osmose in membrane presented many points of similarity to that in earthenware. The membrane is constantly undergoing decomposition and its osmotic action is exhaustible. Salts and other substances, also capable of determining a large osmose, are all chemically active substances, while the great mass of neutral organic substances and perfectly neutral monobasic salts of the metals, such as chloride of sodium, possess only a low degree of action or are wholly inert. The active substances are also relatively most efficient in small proportions. When a solution of the proper kind is used, the osmose or passage of fluid proceeds with a velocity wholly unprecedented in such experiments. The rise of liquid in the tube with a solution containing one-tenth of a per cent. carbonate of potash in the osmometer, was 167 degrees, and with 1 per cent. of the same salt 206 degrees, in five hours. With another membrane and stronger solution, the rise was 863 millimeters, or upwards of 38 inches, in the same time, and as much water was therefore impelled through the membrane as would cover its whole surface to a depth of 8·6 millimeters or one-third of an inch. The chemical action must be

different on the substance of the membrane, at its inner and outer surfaces, to induce osmose; and according to the hypothetic view which accords best with the phænomena, the action on the two sides is not unequal in degree only, but also different in kind. It appears as an alkaline action on the albuminous substance of the membrane, at the inner surface, and as an acid action on the albumen at the outer surface. The most general empirical conclusion that can be drawn is, that the water always accumulates on the alkaline or basic side of the membrane. Hence, with an alkaline salt, such as carbonate or phosphate of soda in the osmometer, and water outside, the flow is inwards. With an acid in the osmometer, on the contrary, the flow is outwards, or there is negative osmose, the liquid then falling in the tube. In the last case the water outside is basic when compared with the acid within, and the flow is therefore still towards the base. The chloride of sodium, chloride of barium, chloride of magnesium, and similar neutral salts, are wholly indifferent, or appear only to act in a subordinate manner to some other active acid or basic substance, which last may be present in the solution or membrane only in the most minute quantity. Salts which admit of dividing into a basic subsalt and free acid exhibit an osmotic activity of the highest order. Such are the acetate and various other salts of alumina, iron and chromium, the protochloride of copper and tin, chloride of copper, nitrate of lead, &c. The acid travels outwards by diffusion, superinducing a basic condition of the inner surface of the membrane and an acid condition of the outer surface, the favourable condition for a high positive osmose. The bibasic salts of potash and soda, again, although strictly neutral in properties, such as the sulphate and tartrate of potash, begin to exhibit a positive osmose, in consequence, it may be presumed, of their possible resolution into an acid supersalt and free alkaline base.

The following Table exhibits the osmose of substances of all classes:—

Osmose of 1 per cent. solutions in Membrane.

	Degrees.		Degrees.
Oxalic acid	−148	Chloride of zinc	54
Hydrochloric acid (0·1 per c.)	− 92	Chloride of nickel	88
Terchloride of gold	− 54	Nitrate of lead	125 to 211
Bichloride of tin	− 46	Nitrate of cadmium	137
Bichloride of platinum	− 30	Nitrate of uranium	234 to 458
Chloride of magnesium	− 3	Nitrate of copper	204
Chloride of sodium	+ 2	Chloride of copper	351
Chloride of potassium	18	Protochloride of tin	289
Nitrate of soda	2	Protochloride of iron	435
Nitrate of silver	34	Chloride of mercury	121
Sulphate of potash	21 to 60	Protonitrate of mercury	356
Sulphate of magnesia	14	Pernitrate of mercury	476
Chloride of calcium	20	Acetate of sesquioxide of iron	194
Chloride of barium	21	Acetate of alumina	280 to 393
Chloride of strontium	26	Chloride of aluminium	540
Chloride of cobalt	26	Phosphate of soda	311
Chloride of manganese	34	Carbonate of potash	439

The osmotic action of carbonate of potash and other alkaline salts is interfered with in an extraordinary manner by the presence of chloride of sodium, being reduced almost to nothing by an equal proportion of that salt. The moderate positive osmose of sulphate of potash is converted into a very sensible negative osmose by the presence of the merest trace of a strong acid, while the positive osmose of the first-mentioned salt is singularly promoted by a small proportion of an alkaline carbonate. The last statement is illustrated by the following observations :—

Osmose in same membrane.

		Degrees.
1 per cent. sulphate of potash		21
Same $+0.1$ per c. carb. potash ..		254
Same $+$ Same		264
0.1 per cent. carbonate of potash alone		92
Same		95

It may appear to some that the chemical character which has been assigned to osmose takes away from the physiological interest of the subject, in so far as the decomposition of the membrane may appear to them to be incompatible with vital conditions, and that osmotic movement must therefore be confined to dead matter. But such apprehensions are, it is believed, groundless, or at all events premature. All parts of living structures are allowed to be in a state of incessant change, of decomposition and renewal. The decomposition occurring in a living membrane, while effecting osmotic propulsion, may possibly therefore be of a reparable kind. In other respects chemical osmose appears to be an agency particularly adapted to take part in the animal œconomy. It is seen that osmose is peculiarly excited by dilute saline solutions, such as the animal juices really are, and that the alkaline or acid property which these juices always possess is another most favourable condition for their action on membrane. The natural excitation of osmose in the substance of the membranes or cell-walls dividing such solutions seems therefore almost inevitable.

In osmose there is further a remarkably direct substitution of one of the great forces of nature by its equivalent in another force—the conversion, as it may be said, of chemical affinity into mechanical power. Now what is more wanted in the theory of animal functions than a mechanism for obtaining motive power from chemical decomposition as it occurs in the tissues? In minute microscopic cells, the osmotic movements should attain the highest velocity, being entirely dependent upon extent of surface. May it not be hoped, therefore, to find in the osmotic injection of fluids the deficient link, which certainly intervenes between muscular movement and chemical decomposition?

XXII. *Intelligence and Miscellaneous Articles.*

ON THE OCCURRENCE OF ZINC IN THE VEGETABLE ORGANISM.
BY A. BRAUN.

IT is well known that the calamine hills of Rhenish Prussia and the neighbouring parts of Belgium possess a peculiar flora; visitors to these regions are particularly surprised by a species of violet allied to *Viola tricolor*, which unfolds its beautiful yellow flowers in uninterrupted profusion from spring until the end of autumn, and is known in the neighbourhood of Aix as the Calamine violet, or in the dialect of the district "Kelmesveilchen." This plant has been described by Lejeune in his "Revue de la Flore de Spaa" as a distinct species under the name of *Viola calaminaria*, but he has since characterized it (*Comp. Floræ Belgicæ*) as *Viola lutea*, Smith. Koch and other authors have also rightly considered it as a variety of *V. lutea*, Smith (*grandiflora*, Huds.), a species principally distinguished from *V. tricolor* by its filiform subterraneous runners, by means of which it survives the winter. In its habits it is remarkably distinct from the ordinary *Viola lutea* of the Alps, as well as from the form of this plant occurring on the higher Vosges on granitic and syenitic soils (described by Spach as *Viola elegans*); its stem being more procumbent and repeatedly branched at the base, and the flowers being generally smaller. I will not, however, express any further opinion as to whether this violet may or may not be a distinct species, for the violets of the same group as *V. tricolor* present so many difficulties to systematic botanists in consequence of their extraordinary variability, that it is difficult to find the middle course between the union of them all under one name, and the establishment of a multitude of species. Many other plants grow in company with the *Viola calaminaria*, which, although in this district peculiar to the calamine hills, nevertheless grow in other localities in soil free from calamine.

The colour of the flowers of the *Viola lutea* of the Alps and Vosges varies from the darkest violet to the purest yellow, whilst the flowers of *V. calaminaria*, at least in the neighbourhood of Aix, are almost always yellow. On the borders of the calamine district specimens are met with here and there with pale violet, or bluish, or mixed blue and yellow flowers, which have been regarded by Kaltenbach as hybrids between this plant and the *V. tricolor*, which certainly occur on cultivated land in the neighbourhood. But I have also seen a specimen of the true *V. calaminaria* from the calamine region of Westphalia which is of a dark violet colour. The plant when cultivated in gardens is said to change and become like the common *V. tricolor*.

The connexion between the occurrence of the *V. calaminaria* and the presence of calamine in the soil, which is so constant that even mining experiments have been undertaken with good results from the indications furnished by this plant, induced me, when in Aix, to urge M. Victor Monheim of that place, to examine the plant especially with reference to its containing zinc. He afterwards

sent me the following account of a chemical investigation of the plant, performed in his laboratory and under his eye, by M. F. Bellingrodt, which I give in the latter gentleman's own words:—

"The plants, some of which were still in flower, were collected in the month of October on the Altenberg and in the immediate neighbourhood of its large zinc works. To get rid of adhering earth completely, the fresh, uncut herbage with the roots was washed with water, until, when macerated for sixteen or eighteen hours with water containing muriatic acid, it gave no inorganic matter to the dilute acid. The whole was then finely chopped and digested on the vapour-bath for twelve hours with water and muriatic acid; the vegetable matter was separated from the extract, and this treated with chlorate of potash. The addition of an excess of ammonia to the decolorized extract, now produced a precipitation of alumina, organic substances, and partially of the iron.

"The precipitate produced in the filtrate by sulphuret of ammonium was dissolved in muriatic acid, oxidized by nitric acid, and the iron then completely separated by ammonia. A portion of the filtered fluid was boiled with solution of potash, when traces of manganese were precipitated. Solution of sulphuretted hydrogen then rendered the presence of *zinc* in the filtrate quite evident.

"Another portion of the fluid filtered from the iron precipitate was precipitated at once by sulphuret of ammonium, the dried precipitate calcined in a platinum crucible, moistened with nitric acid, again calcined, and then treated with dilute acetic acid; the zinc was precipitated from the solution in acetic acid by solution of sulphuretted hydrogen.

"From another portion of the herb, freed from external impurities, the juice was expressed, and the presence of zinc in this was also distinctly proved by the above process."

This metal must therefore be added to the eighteen elements hitherto known to occur in the vegetable organism.—Poggendorff's *Annalen,* vol. xcii. p. 175.

ON THE COMPOSITION OF TANNIC ACID. BY A. STRECKER.

It appears from my experiments, that tannic acid and the tannins in general are much more complex bodies than is generally supposed. In fact, by the action of mineral acids, of alkalies, or of ferments, they are resolved into glucose and a new acid by fixing the elements of water. This resolution, which I announced two years ago, has served me as a starting-point in the determination of the molecule of tannic acid.

According to the analyses of Pelouze, Liebig and Berzelius, the molecule of tannic acid is expressed by the formula $C^{18} H^8 O^{12}$, and it is supposed that in its neutral salts 3 equivs. of water of this formula are replaced by 3 equivs. of metallic oxide. There is nevertheless only one tannate (that of lead) which appears from analysis to contain the carbon and metal in the proportion of 18 : 3 equivs.

Perceiving from the splitting of tannic acid into glucose ($C^{12} H^{12} O^{12}$) and gallic acid ($C^{14} H^6 O^{11}$) that the above formula could not express

the molecule of acid, I undertook a series of experiments to deter-mine the true formula of tannic acid ; of these I now lay the results before the Academy.

To obtain tannic acid in a pure state, I purified the acid prepared by the method of M. Pelouze, in two ways ; one portion was dis-solved in pure æther and the solution precipitated by water, the other portion was dissolved in water and precipitated by æther. Under these conditions two or three distinct strata are obtained, of which the heaviest consists of tannic acid, dissolved in the ætherial fluid. This syrupous liquid was dissolved in water and then evapo-rated *in vacuo* ; the residue was analysed after being dried at 248° F. Ten analyses made with oxide of copper in a current of oxygen gas upon substances obtained at seven different preparations, gave results leading to the formula $C^{54} H^{22} O^{34}$.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	Calculated.
C	52·5	52·2	52·2	52·2	52·2	52·3	52·1	52·2	52·3	52·3	54=52·4
H	3·8	3·8	3·7	3·7	3·6	3·9	3·8	3·6	3·7	3·5	22= 3·6
O	...	...	...	...	...	...	...	...	...	...	34=44·0

According to this formula, the resolution of tannic acid into gallic acid and glucose may be represented by the following equation :—

$$C^{54} H^{22} O^{34} + 8HO = 3(C^{14} H^6 O^{10}) + C^{12} H^{10} O^{10}.$$

This equation is confirmed by the quantities of the two compounds obtained by the decomposition of tannic acid. The former was found to be 87 per cent. (maximum) by Wetherill ; of glucose I have obtained as much as 22 per cent.

The quantity of water which may be displaced by metallic oxide in the molecule $C^{54} H^{22} O^{34}$ I have determined directly by digesting tannic acid with oxide of lead, and the analysis of the neutral and basic salts. By the former method I found that the acid loses 4·4 per cent., or 3 equivs. of water, which is confirmed by the analysis of the salts of lead prepared by precipitation, in which the compo-sition of the anhydrous acid is represented by the formula $C^{54} H^{19} O^{31}$, differing by 3 equivs. of water from the formula $C^{54} H^{22} O^{34}$. The precipitates obtained by tannic acid and acetate of lead contain from 3 to 10 equivs. of oxide of lead, in proportion to C^{54}. The analyses of tannates made by Pelouze, Liebig, Berzelius, Mulder and Büchner, agree with the new formula of tannic acid, if we suppose that some salts do not lose all their water at 212° F., or that they were not completely dried.

According to Berzelius, tannic acid combines with sulphuric or muriatic acid, when these acids are added to a solution of tannic acid in water. In these cases precipitates are obtained which are dissolved at the commencement, until the fluid contains an excess of sulphuric or muriatic acid. These compounds are distinguished from the conjugate acids, as the mineral acid can be separated there-from by salts of baryta or silver. The analysis of these precipitates shows that they are nothing but tannic acid imbued with the acid liquor in which they are deposited. In fact, this sulphuric tannin only contains from 2 to 4 per cent. of sulphuric acid ; and the mu-riatic tannin, when placed *in vacuo* over quicksilver, loses its muriatic

acid completely. Moreover, these precipitates differ in no respects from tannic acid, which has been acidulated with a mineral acid. The formation of a precipitate by a mineral acid in a solution of tannin is therefore perfectly conformable to the precipitation of the same solution by chloride of sodium and other alkaline salts, and even by æther, and must be explained by a diminution of the solubility of the tannin in consequence of the change in the solvent.

Gallic acid, which, according to some chemists, contains in the formula $C^{11} H^6 O^{10}$ 4 or 2 equivs. of water, is, from my analyses, a tribasic acid; and the yellow lead salt, regarded by Liebig and Büchner as a neutral salt, and represented by the formula

$$4PbO . C^{14} H^2 O^6,$$

is a basic salt, which, when dried at 248° F., has a composition represented by the formula

$$4PbO . C^{14} H^3 O^7, \text{ or } 3PbO . C^{14} H^3 O^7 + PbO.$$

In fact, five analyses performed with materials prepared at different times gave 75·9 to 76·1 per cent PbO; numbers which approach much more closely to the latter formula than to that of Liebig and Büchner.—*Comptes Rendus,* July 3, 1854, p. 49.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1854.

Chiswick.—June 1. Fine: cloudy. 2. Rain. 3. Cloudy: clear. 4. Cloudy: clear and cold at night. 5. Uniformly overcast: fine. 6. Cloudy and cold. 7, 8. Cloudy. 9. Overcast. 10. Fine. 11. Fine: overcast. 12. Densely clouded: slight shower: very clear at night. 13. Rain: clear. 14. Cloudy: very fine. 15. Hazy and drizzly. 16. Hazy: rain. 17. Uniformly overcast: very fine: clear. 18. Clear and very fine. 19. Very fine: cold at night. 20. Slight haze: cloudy: fine: clear. 21. Fine: cloudy: rain. 22. Very fine. 23. Uniformly overcast: very fine: clear. 24, 25. Cloudy and fine. 26. Fine. 27. Fine: rain at night. 28. Showery: very clear. 29. Cloudy: clear. 30. Very fine: rain at night.

Mean temperature of the month .... 56°·93
Mean temperature of June 1853 .... 59 ·16
Mean temperature of June for the last twenty-eight years . 60 ·51
Average amount of rain in June .... 1·90 inch.

Boston.—June 1. Cloudy. 2. Rain A.M. 3—11. Cloudy. 12. Cloudy: rain A.M. 13—15. Cloudy. 16. Cloudy: rain A.M. 17, 18. Cloudy. 19—22. Fine. 23. Fine: thermometer 83° half-past 2 P.M. 24. Fine: thermometer 80° half-past 2 P.M. 25. Fine: thermometer 87°: rain P.M. 26. Cloudy: rain A.M. 27. Cloudy. 28. Cloudy: rain A.M. and P.M. 29. Cloudy. 30. Cloudy: rain P.M.

Sandwick Manse, Orkney.—June 1. Drizzle A.M. and P.M. 2. Cloudy A.M. and P.M. 3. Bright A.M.: cloudy P.M. 4. Cloudy A.M. and P.M. 5. Damp A.M. and P.M. 6. Cloudy A.M. and P.M. 7. Cloudy A.M.: drizzle P.M. 8. Damp A.M.: cloudy, fine P.M. 9. Drops A.M.: small rain P.M. 10. Cloudy A.M.: drops P.M. 11. Clear A.M.: rain P.M. 12. Clear A.M.: clear, fine P.M. 13. Clear A.M.: drops P.M. 14. Cloudy A.M.: showers, cloudy P.M. 15. Cloudy A.M.: clear P.M. 16. Bright A.M.: cloudy P.M. 17. Clear A.M.: cloudy P.M. 18. Damp A.M.: cloudy P.M. 19. Rain A.M.: fog P.M. 20. Clear A.M. and P.M. 21. Clear, fine A.M. and P.M. 22. Bright A.M.: cloudy P.M. 23. Drizzle A.M.: cloudy, fine P.M. 24. Rain A.M.: clear, fine P.M. 25. Clear, fine A.M.: cloudy, fine P.M. 26. Showers A.M.: damp P.M. 27. Showers A.M.: cloudy P.M. 28. Clear, fine A.M.: cloudy P.M. 29. Clear, fine A.M.: drizzle P.M. 30. Drizzle A.M.: drizzle, showers P.M.

Mean temperature of June for twenty-seven previous years . 52°·81
Mean temperature of June 1853 .... 55 ·21
Mean temperature of this month .... 52 ·86
Average quantity of rain in June for thirteen previous years . 2·24 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1854. June.	Barometer. Chiswick. Max.	Chiswick. Min.	Boston. 8¼ a.m.	Orkney, Sandwick. 9¼ a.m.	Orkney, Sandwick. 8½ p.m.	Thermometer. Chiswick. Max.	Chiswick. Min.	Boston. 8¾ a.m.	Orkney, Sandwick. 9¼ a.m.	Orkney, Sandwick. 8½ p.m.	Wind. Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Rain. Chiswick.	Boston.	Orkney, Sandwick.
1.	29·916	29·830	29·60	30·08	30·13	67	50	58	49	47½	ne.	e.	ne.	·04		·04
2.	29·775	29·679	29·43	30·09	30·11	57	47	50	50	46½	ne.	n.	ne.	·05	·03	
3.	29·916	29·775	29·44	30·14	30·17	60	44	54	50½	46	ne.	n.	wnw.	·02		
☽ 4.	30·125	30·036	29·74	30·19	30·22	62	36	56	48	47½	ne.	nne.	nw.			
5.	30·110	30·038	29·76	30·22	30·24	62	49	54	49½	48½	ne.	ne.	nw.			
6.	30·083	30·040	29·70	30·21	30·19	59	47	54	50½	49½	n.	nne.	nw.			
7.	30·070	30·043	29·72	30·13	30·10	57	45	52	52½	49	ne.	n.	nw.			
8.	30·073	29·980	29·67	30·04	29·96	65	48	57	48	49½	ne.	wnw.	calm			
9.	29·961	29·929	29·56	29·84	29·72	64	46	55	54	48½	ne.	nw.	calm			
○ 10.	29·908	29·881	29·44	29·59	29·63	66	40	57	48	48	w.	w.	wnw.			·24
11.	29·870	29·654	29·35	29·41	29·08	65	55	61	52	51½	sw.	ssw.	sse.	·01		
12.	29·642	29·565	29·13	29·19	29·24	67	46	60	57	51	sw.	ssw.	sse.			·13
13.	29·642	29·585	29·10	29·32	29·24	65	47	59	57	51	sw.	ssw.	ese.		·35	
14.	29·725	29·670	29·15	29·27	29·47	66	52	62	54½	51½	w.	sw.	sw.	·14		·05
15.	29·720	29·662	29·30	29·62	29·80	60	50	65	53	51½	e.	ne.	sw.	·04		·04
16.	29·772	29·622	29·40	29·78	29·84	67	53	66	55½	51	sw.	ne.	ese.	·04		
☾ 17.	29·657	29·569	29·10	29·87	29·85	70	45	65	54½	51½	s.	sw.	ne.	·45	·12	
18.	29·856	29·743	29·33	29·86	29·94	66	38	59	51	51½	s.	s.	ese.			
19.	29·929	29·912	29·44	29·80	29·72	66	44	59	54	51½	sw.	sw.	se.			·40
20.	29·908	29·876	29·46	29·81	29·80	67	46	60	57	52	sw.	w.	ese.			·47
21.	30·030	30·008	29·52	29·77	29·83	72	59	63	57	56	sw.	sw.	e.	·06		
22.	30·077	30·067	29·68	29·91	29·88	77	52	66	60	56	sw.	w.	se.			
23.	30·139	30·103	29·60	29·85	30·01	77	52	67	57	55	sw.	w.	e.			
24.	30·159	30·078	29·60	29·95	29·89	81	61	69	56½	63½	s.	w.	calm			·17
● 25.	29·987	29·853	29·45	29·79	29·77	68	48	71	61½	57	sw.	s.	wnw.	·01		
26.	29·690	29·651	29·14	29·65	29·46	65	49	74	57	56	sw.	sse.	ese.	·17	·48	·10
27.	29·790	29·725	29·25	29·42	29·61	68	43	60	57	53	sw.	s.	sse.	·06		·21
28.	29·649	29·611	29·22	29·74	29·85	71	49	66	56	55	sw.	s.	e.			
29.	29·615	29·590	29·18	29·89	29·85	71	41	60	56	54	sw.	w.	nw.		·27	
30.	29·803	29·666	29·25	29·85	29·85	70	51	63	56	50	w.	nw.	n.	·44		
Mean.	29·885	29·814	29·44	29·809	29·815	66·23	47·63	60·9	53·96	51·76				1·53	1·25	1·85

XXIII. *On the Action of the Ferment of Madder on Sugar.*
By Edward Schunck, *Ph.D., F.R.S.*[*]

IN a paper read before the Royal Society in 1852[†], I have given an account of the nature and results of the peculiar process of fermentation which takes place in madder and its watery extracts, when exposed to a moderate degree of temperature. I have shown that during this process the bitter principle of madder, to which I have given the name of *rubian,* is completely decomposed, giving rise to the formation of a number of substances, of which alizarine, the true colouring matter of madder, is the most remarkable. I have stated that this process is not accompanied by the evolution of gas or any of the usual signs of fermentation, that the access of atmospheric air is not necessary for its completion, and that the rapidity with which it is effected is remarkable. Lastly, I have given an account of the properties and composition of the ferment itself. I have shown that, as regards the power of effecting the decomposition of rubian, none of the usual fermentative substances, such as yeast, caseine, &c., are capable of supplying its place, with the sole exception of emulsine, which forms an imperfect substitute for it; that its composition differs in a marked manner from that of the other bodies of the same class in containing a much smaller proportion of nitrogen; and that, in short, it must be considered as a substance altogether *sui generis.* I have given it the name of *Erythrozym.*

In the paper just mentioned, I have stated that "if this sub-

stance be mixed with water, and the mixture be allowed to stand for a length of time in a warm place, signs of a more active process of fermentation begin to show themselves, especially in summer weather; bubbles of gas are given off, and a peculiar smell is emitted, which, though disagreeable, cannot exactly be called putrid. During this process, which is evidently one of putrefaction in the stricter sense, the erythrozym loses its sliminess, and is converted into a red flocculent mass, which may easily be separated by filtration from the liquid. The latter is clear, colourless, and quite neutral. After erythrozym has passed through this second stage of decomposition, its power of decomposing rubian is found to have lost much of its intensity. It is during the first period of its decomposition, when no apparent change is taking place, that this power is most energetically exerted. During the second, or more strictly putrefactive stage, it acquires, however, the property of decomposing sugar. If erythrozym be mixed with a solution of cane-sugar, and the mixture be allowed to stand for a considerable time until gas begins to be disengaged, the solution acquires by degrees a decided acid reaction." The great interest attaching to bodies of this class induced me to examine this action of the ferment on sugar more minutely, and I have now the honour of laying the results of my investigation before the Society.

In order to obtain the ferment for the purpose of decomposing sugar, it is not necessary to prepare it by precipitation with alcohol, as I recommended in my paper on rubian, nor to pay much attention to its complete purification. I found that the following method of preparation yielded a product perfectly well adapted for the purpose. A quantity of madder (French being the kind employed) having been placed on a calico strainer, water heated to about 100° F. is poured on it in the proportion of about four quarts of water to every pound of madder. To the dark reddish-brown liquid there is now added a small quantity of muriatic acid. This produces a brown flocculent precipitate, which is allowed to settle, and after the liquid has been decanted, it is washed with a quantity of fresh water, the process being repeated until the excess of acid has been removed, after which the precipitate is collected on a calico strainer. After the water has run off, there is left on the strainer a thick brown pulp. This pulp contains, besides erythrozym, small quantities of pectic acid, colouring matter, &c., but these impurities are of no material consequence as regards the process of fermentation. It differs from the erythrozym prepared by precipitation with alcohol in containing no lime or other base, the latter having been removed by the acid used for precipitation. In my experiments, however, I always added a certain proportion of lime-

water to the solutions to be fermented, as I found that the fermentation was much promoted by this addition.

Most of my experiments were made with cane-sugar, but I have also subjected grape- and milk-sugar to the same process of decomposition. In operating on cane-sugar I proceeded in the following manner. The sugar having been dissolved in water, I added to the solution for every pound of sugar taken about four quarts of the brown pulp, prepared as just described, and a sufficient quantity of lime-water to change the colour of the ferment from brown to dark purple, and to cause a slight alkaline reaction in the liquid. The whole having been well mixed, was left to itself. The vessels which I employed for conducting the operation in, were large earthenware mugs. In warm summer weather, provided the quantity of materials taken was not too small, the fermentation generally commenced on the succeeding day. A copious disengagement of gas took place, and continued for a number of days. The bubbles of gas, in rising, formed, together with particles of ferment carried up by them, a thick scum on the surface of the liquid, resembling the froth on the surface of fermenting beer. As far as the evolution of gas, however, was concerned, the action did not seem to be quite as energetic as would have been produced by the action of ordinary yeast on sugar. During this stage of the fermentation a slight vinous odour might be perceived near the surface of the liquor. After a few days the liquid began to acquire a decided acid reaction, which was first indicated by the colour of the ferment changing from purple to brown. Very soon litmus paper began to be strongly reddened by it. After some time the disengagement of gas ceased, and the particles of ferment all sank to the bottom, leaving a clear, yellowish, supernatant liquid. The latter, however, on standing exposed to the atmosphere, seemed constantly to acquire more and more acid properties; and after several weeks' exposure, it was generally found to have a strongly acid taste, and smell like that of sour beer. I have generally allowed the mixture to stand for several months, usually from summer to winter, as I imagined that long standing promoted the formation of one of the acid products, whose properties I shall presently describe. Nevertheless, during this lapse of time little or no mould was formed on the surface of the liquor, no decidedly putrid smell was emitted, nor did the mass ever seem to be particularly attractive to insects, or to breed worms or larvæ, at least none that were visible to the naked eye.

Now the products of this process of fermentation are of three kinds; gaseous, liquid, and solid. I shall describe them in the order mentioned. For the purpose of examining the gaseous

products, 2 lbs. of cane-sugar were dissolved in water, and the usual quantities of ferment and lime-water were added.   After two days the mixture was found to be in a state of active fermentation.   A portion of it was then introduced into a capacious bottle, leaving a small space at the top, which was filled up with oil in order to prevent frothing.   The gas was allowed to escape through a bent tube carried through the cork.   In order to expel all the air which might be left in the apparatus, the disengagement of gas was allowed to proceed for a whole day before any of it was collected for examination.   Six cubic inches were then collected in a graduated tube over mercury.   Caustic soda was then introduced into the tube in order to absorb the carbonic acid.   The residual gas consisted almost entirely of hydrogen. Its amount was determined by means of oxygen and spongy platinum.   After making the necessary corrections for temperature and pressure, the 6 cubic inches of mixed gas were found to consist of 4·90 carbonic acid, 0·96 hydrogen, and 0·14 atmospheric air or nitrogen.   The day afterwards I collected 5·16 cubic inches of gas, and found it to contain 4·41 carbonic acid and 0·75 hydrogen.   Three days later again, 5·77 cubic inches of gas were collected, and found to be composed of 4·86 carbonic acid, 0·76 hydrogen, and 0·15 atmospheric air or nitrogen. According to these three determinations, the proportion of hydrogen to carbonic acid was respectively as 1 : 5·10, 1 : 5·88, and 1 : 6·39.   It appears, therefore, that the carbonic acid increased in quantity relatively to the hydrogen, as the fermentation proceeded.

The liquid products of the fermentation were examined in the following manner.   A solution of 4 lbs. of cane-sugar was mixed with 19 quarts of brown pulp, to which lime-water had been previously added in the usual proportion.   The mixture was allowed to ferment for about two weeks, during which time much gas was disengaged.   The liquid, which had acquired an acid reaction, was then strained through calico, after which it measured 36 quarts.   These 36 quarts were introduced into a still, and distilled until 8 quarts had passed over.   By a second distillation I obtained 2 quarts, and by a third distillation 12 fluid ounces of a liquid, which was inflammable, alcoholic in taste and smell, devoid of acid reaction, and having a specific gravity of 0·973.   Assuming it to consist entirely of alcohol and water, of which there is little doubt, the 12 fluid ounces of this specific gravity would correspond to 1072½ grs. of absolute alcohol. The only peculiarity which I noticed about it as distinguishing it from ordinary spirits of wine from other sources, was, that it appeared to be filled with a quantity of small, white, glistening, micaceous scales, which gave the liquid when agitated the ap-

pearance of being traversed with silky bands. On filtering, these scales remained on the filter, but their quantity was so insignificant that they could not again be separated from the paper.

The liquid remaining in the retort after the second distillation was added to that remaining after the third distillation, and evaporated together with an excess of carbonate of soda until the bulk was very much diminished. The liquid was then supersaturated with sulphuric acid and distilled. The distillate was colourless, but had a strong acid taste and reaction. It was neutralized with carbonate of soda, and then gave on evaporation a crystalline mass, which was white with a brownish tinge. This mass, when treated with boiling dilute sulphuric acid, evolved a pungent smell like that of acetic or formic acid. Its watery solution gave reactions similar to those of formiate of soda. It gave, for instance, with nitrate of silver a white crystalline precipitate, which soon became black when left to stand, but immediately on boiling the liquid; with protonitrate of mercury, a white crystalline precipitate, which on standing was slowly reduced to gray metallic mercury; and with corrosive sublimate it produced, on boiling, a copious deposit of white crystalline scales (calomel). But on adding acetate of lead, evaporating to dryness, and treating the residue with alcohol, no crystals of formiate of lead were left undissolved. I therefore concluded that the salt consisted for the most part of acetate of soda, contaminated with some impurity, which obscured the reactions proper to acetic acid. Whether the acetic acid thus obtained is a product of the direct action of the ferment on sugar, or whether it is formed indirectly from the oxidation of the alcohol produced in the first instance, is uncertain. The greater part of the acid found in the fermented liquor after exposure for some time, in quantities so considerable as to impart to the liquid a strong acid taste and smell, is without doubt derived from the latter source.

The last product of this process of fermentation which I shall have to mention is solid, and though the most interesting of them all, is formed in such small quantities as to render its identification difficult. In order to obtain an appreciable quantity of it, it is necessary to employ several pounds of sugar. The solution of sugar being mixed with ferment and lime-water in the proportions stated above, the mixture is allowed to ferment, and to stand for several weeks at least, after the disengagement of gas has ceased. The liquid is then strained through calico, and the ferment which remains on the calico in an apparently unchanged state, is washed with water until the percolating liquid is no longer acid. The liquid is then rendered alkaline

by means of lime-water, and again strained, in order to separate
a small quantity of flocks thrown down by the lime.   Sugar of
lead is now added to it, which produces a dirty, pinkish-white
precipitate.   This is collected on a filter, washed with water,
and decomposed with sulphuretted hydrogen.   The acid liquid
filtered from the sulphuret of lead is usually dark brown or
black, from sulphuret of lead in a state of suspension.   During
evaporation, however, it deposits this sulphuret of lead, and after
being again filtered is clear, though still very brown.   After
being evaporated almost to a syrup, milk of lime is added to it,
and the mixture is boiled.   The lime removes a quantity of
phosphoric acid, which is probably derived from undecomposed
phosphates contained in the ferment, and also a great part of
the brown colouring matter.   Through the filtered liquid, which
is much lighter in colour than before, I now pass a stream of
carbonic acid gas, until the excess of lime is completely neutral-
ized, after which it is evaporated almost to dryness.   The car-
bonate of lime which is deposited during evaporation is separated
by filtration, and the liquid is evaporated with the addition of
an excess of muriatic acid, until it leaves a thick dark brown
syrup.   In this syrup there is formed, after cooling and stand-
ing, a mass of crystals.   These crystals must now be strongly
pressed between folds of blotting-paper, until the whole of the
syrupy mother-liquor containing chloride of calcium and other
impurities has been absorbed.   The crystalline mass left on the
paper, which has a light brown colour, is now treated with boil-
ing alcohol, which leaves undissolved a quantity of sulphate of
lime.   The alcohol, after filtration, is evaporated to dryness, the
residue is redissolved in boiling water, and the solution is deco-
lorized with animal charcoal.   The solution now leaves on eva-
poration to dryness a colourless crystalline mass, consisting
partly of needles and plates, partly of crystalline crusts.   Now
this crystalline mass is found to possess the properties of an acid
in a very marked manner.   Its taste is at first strongly acid, but
this is immediately followed by a nauseous, somewhat metallic
after-taste.   Its solutions redden litmus-paper strongly.   When
heated on platinum-foil it melts and burns with a very pale
flame, leaving a slight carbonaceous residue.   When heated in
a glass tube it melts, and crystallizes again on cooling.   When
further heated it is volatilized, yielding fumes which strongly
affect the nostrils and throat, produce a choking sensation, and
excite violent coughing.   The fumes condense on the colder
parts of the tube, partly in the shape of needles, partly as a
white, crystalline, radiated mass. Very little carbonaceous residue
is left, and even this is probably due to impurities, so that the
acid may be considered as completely volatile.   The watery

solution of the acid gives the following reactions. On adding lime-water until the solution is alkaline, no precipitate is produced. On boiling, a very slight flocculent deposit is formed. The filtered solution leaves on evaporation a crystalline mass, which dissolves again for the most part in boiling water, leaving undissolved only a small quantity of carbonate of lime. The filtered solution is neutral to test-paper, and on being again evaporated, leaves a quantity of long white needles, consisting, without doubt, of the lime salt. The taste of these crystals is nauseous. When heated in a glass tube they become black and give fumes, but no crystalline sublimate; the residue dissolves in acids, with effervescence, leaving some carbon undissolved. On adding baryta water to the watery solution of the acid, there is formed immediately a white flocculent precipitate, which is soluble in muriatic acid, and is again formed on neutralizing the acid with ammonia. Perchloride of iron gives no precipitate in the watery solution of the acid, but in the solution of the lime salt it gives a copious light brown precipitate. Acetate of lead gives immediately a slight flocculent precipitate. If the liquid be filtered from this precipitate, and be allowed to stand for a day or two, there are formed on the bottom and sides of the vessel a number of lustrous, well-developed crystals of a rhombohedral form. The liquid yields on evaporation no more crystals, but only a syrup of acetate of lead. Nitrate of silver produces no precipitate in the watery solution of the acid, but on the addition of ammonia, a copious white precipitate is formed, which, on standing, becomes somewhat crystalline. This precipitate is soluble in nitric acid and ammonia. Acetate of copper gives almost immediately a blue crystalline precipitate, which increases in quantity on standing. It is not soluble in boiling water, but dissolves in acetic acid. These reactions coincide in every, even the minutest, particular with those of succinic acid, a fact of which I have convinced myself by a comparative examination of succinic acid derived from the usual source. The analysis of the acid and its silver salt gave results which leave no doubt of its perfect identity with succinic acid.

0·2795 grm. of the crystallized acid, dried at 212° F. and burnt with chromate of lead, gave 0·4175 grm. carbonic acid and 0·1480 grm. water.

These numbers correspond to the following composition :—

					Succinic acid $C^4 H^3 O^4$ contains
Carbon	.	.	.	40·73	40·67
Hydrogen	.	.	.	5·88	5·08
Oxygen	.	.	.	53·39	54·25
				100·00	100·00

The silver salt was prepared by dissolving the acid in water, then adding nitrate of silver, and neutralizing with ammonia. The white granular precipitate which fell was collected on a filter, washed with water, and dried *in vacuo*, until its weight remained uniform, after which it was submitted to analysis.

0·6275 grm. of the salt, burnt with chromate of lead, gave 0·3400 grm. carbonic acid and 0·0890 grm. water.

0·2850 grm. gave 0·2410 grm. chloride of silver.

These numbers lead to the following composition :—

		Succinate of silver $C^4 H^2 O^3 + AgO$ contains
Carbon . . .	14·77	14·46
Hydrogen . .	1·57	1·20
Oxygen . . .	15·29	14·46
Oxide of silver .	68·37	69·88
	100·00	100·00

It will be seen that the composition, as determined by these analyses, is only an approximation to what it should be by calculation. Both analyses show an excess of carbon, and a still greater of hydrogen, while the amount of oxide of silver is deficient. Such discrepancies are almost unavoidable in the analysis of substances, which like this are obtained in such extremely minute quantities, and which it is consequently almost impossible to bring into a state of perfect purity without losing nearly the whole quantity obtained[*]. Were the atomic weight of the acid yielded by this process much higher, there might still be doubts concerning its composition and identity ; but as the amount of discrepancy between the calculated composition and that found by experiment does not, in the case of any one of the constituents of either the acid or the silver salt, correspond to more than half an equivalent[†], and as the reactions of the acid agree so entirely with those characteristic of succinic acid, I think there can be no room for any uncertainty.

Besides this acid, I have not been able to discover any other solid product of decomposition resulting from this process. Through the liquid filtered from the lead precipitate containing the succinic acid I have passed sulphuretted hydrogen until all

[*] In order to procure the acid used for the two analyses given above, I was obliged to subject about 26 lbs. of sugar to fermentation. The time and labour required to obtain a sufficient quantity of ferment for the purpose (about 100 quarts) are very great.

[†] In the analysis of the acid given above, the quantities of C, H and O, are to one another as $24 : 3·4 : 31·4$, or expressed in numbers of equivalents as $4C : 3·4H : 3·9O$. The amount of the different constituents found in the silver salt are to one another as $24 : 2·5 : 24·8 : 111$, or expressed in equivalents as $4C : 2·5H : 3·1O : 0·95 AgO$.

the lead was precipitated, and then filtered and evaporated. A sweet brown syrup was left, consisting apparently of undecomposed sugar, which, though allowed to stand for a length of time, yielded no trace of anything crystalline. Part of this syrup being redissolved in water, and the solution being again evaporated with the addition of acetate of zinc, no crystals were formed, and the residue left after evaporation was completely soluble in alcohol—a proof of the absence of lactic acid.

The products resulting from this process of decomposition are therefore the following:— carbonic acid, hydrogen, alcohol, acetic acid, and succinic acid. Of these the last-named is the most remarkable; and its formation, though it has been observed in other processes of fermentation, is still so uncommon as to call for some further observation.

I have discovered that on allowing grape-sugar, or sugar of milk, to ferment together with erythrozym and lime-water in the same way as cane-sugar, succinic acid is also formed. Indeed, from sugar of milk I obtained in one experiment more than three times as much of this acid as was ever afforded under the most favourable circumstances by the same quantity of cane-sugar. From 1 lb. of cane-sugar I never obtained more than $3\frac{1}{2}$ grs. of acid. In the experiment just referred to, I obtained from $\frac{1}{2}$ lb. of sugar of milk nearly 6 grs. of acid. The quantities formed, even under apparently the same circumstances, were, however, very variable. Large quantities of cane-sugar sometimes yielded only traces of acid, while smaller quantities gave proportionally a large amount. It is during the later stages of the fermentation, I imagine, that the acid is chiefly formed. At least, I have always found that its amount was greater after the fermenting liquid had stood for several weeks or months than during the first period of the fermentation, when the disengagement of gas was most active. Whether the access of atmospheric air is necessary for, or promotes the formation of, the succinic acid, I am unable to say. I may mention, that the phænomena attending the fermentation of grape-sugar and sugar of milk are apparently the same as when cane-sugar is employed.

In order to be quite sure that the formation of the succinic acid and the other products was due to the action of the ferment on the sugar, and not to the decomposition of the ferment itself, I have repeatedly allowed mixtures of the ferment and lime-water, without the addition of sugar, to stand exposed to a warm temperature and under the same conditions under which the fermentation was usually conducted. In this case little or no gas was disengaged, the liquid never became acid, and the ferment remained purple and sank to the bottom of the vessel. After standing for several weeks, the filtered liquid gave with sugar of

lead a pinkish-white precipitate, which seemed, however, to con-
sist principally of carbonate of lead, as on treating a small quan-
tity of it with nitric acid it dissolved with effervescence.   This
precipitate being decomposed with sulphuretted hydrogen, and
the liquid being treated in exactly the same manner as that
resulting from the crude succinate of lead in the other experi-
ments, a syrup was obtained, which yielded, however, not a trace
of crystalline sublimate when heated in a tube.   The formation
of the succinic acid, as well as of the alcohol and the gases, is
therefore clearly due to the decomposition of the sugar induced
by the action of the ferment.

Since madder itself contains a considerable quantity of sugar
ready formed, and an additional quantity is always produced by
the decomposition of rubian, it was natural to suppose that
watery extracts of madder might be found to contain succinic
acid.   For the purpose of ascertaining whether this was the case,
I took some of the brown syrup which had been obtained by
extracting French madder with boiling water, precipitating the
colouring matter, &c. with oxalic acid, neutralizing the excess of
acid with carbonate of lime, and evaporating the filtered liquid,
which had been standing in the state of syrup for several years.
A quantity of this syrup having been mixed with water, I added
to it acetate of lead, which produced a dark brown precipitate.
This precipitate being treated in precisely the same manner as
the lead precipitate thrown down by sugar of lead from the solu-
tions of sugar fermented with erythrozym, yielded a small quan-
tity of a white crystallized acid, which, when heated in a tube, was
completely volatilized, giving a beautiful crystalline sublimate,
exactly resembling that produced by succinic acid.   It is there-
fore very probable that succinic acid is either contained as such in
madder, or is formed during or after the process of extraction by
the action of the ferment on the sugar contained in the extract.

A few years ago the only known sources of succinic acid were
amber, turpentine, and some species of brown coal and retin-
asphalt.   Latterly it has been discovered in several plants, such
as the *Lactuca sativa* and *virosa**, and the *Artemisia Absinthium*†,
and it has been detected in the liquid extracted from cysts con-
taining Echinococci from the human liver‡.   It has moreover
been produced artificially by the action of nitric acid on different
kinds of fat and fatty acids, such as tallow, wax, spermaceti, and
stearic and margaric acids§ ; and its formation during the fer-

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* Köhnke, *Brandes Archiv*, 2 ser. vol. xxxix. p. 153.
† Zwenger, *Annalen der Pharmacie*, vol. xlviii. p. 122.
‡ Heintz, Poggendorff's *Annalen*, vol. lxxx. p 114.
§ Bromeis, Sthamer, Radcliffe, Ronalds, *Annalen der Pharmacie*, vol. xxxv.
p. 90; vol. xliii. pp. 346, 349, 356.

mentation of asparagine, malic acid, and their compounds, is one of the most interesting facts of organic chemistry. As far as I know, however, its direct formation from sugar has not hitherto been observed, and I consider this as the most important fact revealed by this investigation. In future, should its presence be detected in any part of the vegetable or animal organism, its origin need no longer be a subject for doubtful speculation, as it is now known to be a product of the decomposition of sugar, whether it be cane-sugar, grape-sugar, or sugar of milk*.

I shall venture, in conclusion, to offer a few remarks on the general nature of the process of fermentation here described, and its relation to other processes of the same kind previously known.

The highly-interesting and peculiar class of bodies called ferments, comprises substances which are all of a very complex nature, and are at the same time not characterized by any marked peculiarities in their appearance, form, or general properties. It is chiefly by their action on other bodies, by the different species of decomposition which they induce in the latter, and by the nature of the products thereby formed, that we are enabled to distinguish the ferments from one another, and arrange them in different classes. Now the effects or species of decomposition produced by ferments are of two kinds—general and specific. The general effects are those produced by all ferments, without distinction, or are common to several classes of ferments. The specific effects are those peculiar to each ferment alone. The general effects are again of different kinds; some being produced during the first stages of the fermentation, others when the pro-

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* The only indication which I can find of the formation of succinic acid from sugar having previously been observed, is a statement of Beissenhirtz (*Berlinisches Jahrbuch der Pharmacie,* anno 1818, p. 158), who allowed a mixture of honey, bread, *Siliqua dulcis* (the fruit of the *Ceratonia siliqua*), vinegar, spirits of wine, and water to ferment, neutralized the acid with lime, and then subjected the solution of the lime salt to distillation, together with oxide of manganese and sulphuric acid, when he obtained first a distillate of acetic acid, and afterwards a sublimate of succinic acid. Here the *Siliqua dulcis* probably yielded only the ferment, since John could discover in it no succinic acid ready formed, and the honey the sugar acted on. Plümacher repeated this experiment, but without success. Piria discovered that asparagine is formed during the germination of the seeds of various Leguminosæ, such as peas, beans and vetches, and that the asparagine, by fermentation, yields succinic acid. Dessaignes could not discover what body contained in the seeds it is, which leads to the formation of asparagine; but he found that pea flour, when allowed to ferment with caseine, produced considerable quantities of succinic acid. It is now evident that this acid may have been formed directly from the starch of the pea flour, though it is possible (and it would be a fact of uncommon interest if it were discovered to be the case) that the latter passes through the intermediate stage of asparagine.

cess has somewhat advanced, others when it approaches a conclusion—these different effects corresponding to different stages of decomposition in the ferment itself. I think I am correct in saying that there are only two well-known instances of specific effects due to ferments. The one is the decomposition of amygdaline (and salicine ?) by means of emulsine, the ferment contained in almonds; the other the decomposition of rubian by means of erythrozym. No known ferment, with the exception of emulsine, has any effect whatever on amygdaline*, and none of the usual ferments, such as yeast, decomposing caseine, albumen, gelatine, or even emulsine, are capable of supplying the place of erythrozym, as far as regards the decomposition of rubian†. These specific effects of emulsine and erythrozym are more characteristic of these bodies than any other property whatever, and serve to distinguish them from one another and from other ferments, with more accuracy than any difference in composition, however great. On the other hand, the decomposition of sugar into alcohol and carbonic acid is an effect common to all known ferments. All so-called proteine compounds, such as albumen, caseine, animal membranes, &c., when they enter into decomposition, acquire the properties of ferments. When all other circumstances are alike, the processes of decomposition to which these compounds, when acting as ferments, give rise in other bodies, are precisely the same. The species of decomposition varies only according to the particular stage of decomposition of the ferment itself. During the first stage of decomposition they convert starch into sugar; during the second stage they change sugar into alcohol and carbonic acid, bile into cholalic acid, taurine, and other products, and tannic into gallic acid; when they have entered on the third stage of decomposition, they effect the conversion of sugar into lactic acid, and of lactic acid into butyric acid, carbonic acid and hydrogen. All these various effects may be produced by emulsine, provided the latter be in the state of decomposition appropriate to their respective production. The alcoholic fermentation of sugar is also effected by erythrozym, as I have shown ‡.

* I have mixed amygdaline and erythrozym, and amygdaline and madder itself, together with water, to the consistence of paste, and allowed the mixtures to stand for days in a warm place, without perceiving any signs of the decomposition of the amygdaline; while a mixture of amygdaline, emulsine, and water will, under the same circumstances, evolve the peculiar smell of oil of bitter almonds in a very short time.

† Emulsine is indeed not entirely without effect on rubian, but the quantity of the latter which it is capable of decomposing, even after a long lapse of time, is extremely insignificant.

‡ It is possible that butyric acid may also be one of the products of the action of erythrozym on sugar, though it is not probable, as among these

But it still remains uncertain, and the point is one of considerable interest, whether the formation of succinic acid from sugar is a specific effect due to erythrozym alone, or is shared by the latter in common with other ferments, such as yeast and emulsine. On this point I have no evidence to offer, but must content myself with a few general considerations, leading to the conclusion that it is not improbable that other ferments will be found capable, under peculiar circumstances, of producing this acid from sugar.

The conversion of sugar into alcohol and carbonic acid, and also that of sugar into butyric acid, carbonic acid and hydrogen, are well-understood processes. In the former case 1 equiv. of sugar splits up into 2 equivs. of alcohol, and 4 equivs. of carbonic acid.

$$\underset{\text{1 equiv. sugar.}}{C^{12} H^{12} O^{12}} = \underset{\text{Alcohol.}}{2C^4 H^6 O^2} + 4CO^2.$$
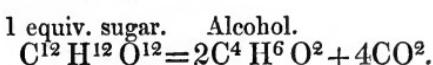
In the second case, 1 equiv. of sugar splits up into 1 equiv. of butyric acid, 4 equivs. of carbonic acid, and 4 equivs. of hydrogen*.

$$\underset{\text{1 equiv. sugar.}}{C^{12} H^{12} O^{12}} = \underset{\text{Butyric acid.}}{C^8 H^8 O^4} + 4CO^2 + 4H.$$
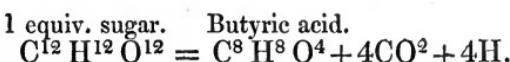
But as regards succinic acid, it is difficult to indicate with positive certainty in what manner it takes its rise from sugar; because, in the fermentation of the latter with erythrozym, it is evident that there are two processes going on side by side, the one being the formation of alcohol, the other that of succinic acid; and it is doubtful whether all the carbonic acid evolved is due to the one process alone or not, and also whether all the acetic acid formed arises from the oxidation of the alcohol, or whether a part of it does not proceed directly from the decomposition of sugar.

That the disengagement of hydrogen in this process stands in some relation to the formation of succinic acid, will admit, I think, of no doubt; but I would not venture to assert that the whole quantity of that gas evolved is due to the process of decomposition by which this acid is formed. Of the possible modes of decomposition by which succinic acid may be formed from sugar, I will indicate a few. 1 equiv. of sugar may split up into 3 equivs. of succinic acid and 3 equivs. of hydrogen; or with the elements of 8 equivs. of water into 1 equiv. of succinic

products I have not been able to detect lactic acid, which constitutes the intermediate stage between sugar and butyric acid.

* I leave out of consideration the fact that sugar, before being decomposed in the manner mentioned, is first converted into lactic acid; but this fact is of no consequence as regards the result, since sugar and lactic acid have the same per-centage composition.

acid, 8 equivs. of carbonic acid, and 17 equivs. of hydrogen ; or into 1 equiv. of succinic acid, 2 equivs. of acetic acid, and 1 equiv. of hydrogen ; or into 2 equivs. of succinic acid, 1 equiv. of acetic acid, and 2 equivs. of hydrogen ; or together with the elements of 2 equivs. of water into 2 equivs. of succinic acid, $\frac{1}{2}$ equiv. of acetic acid, 2 equivs. of carbonic acid, and 8 equivs. of hydrogen, as the following equations will show :—

$$\underset{\text{Succinic acid.}}{C^{12} H^{12} O^{12}} = 3C^4 H^3 O^4 + 3H$$
$$C^{12} H^{12} O^{12} + 8HO = C^4 H^3 O^4 + 8CO^2 + 17H.$$

$$C^{12} H^{12} O^{12} = \underset{\text{Acetic acid.}}{C^4 H^3 O^4} + 2C^4 H^4 O^4 + H$$
$$C^{12} H^{12} O^{12} = 2C^4 H^3 O^4 + C^4 H^4 O^4 + 2H$$
$$C^{12} H^{12} O^{12} + 2HO = 2C^4 H^3 O^4 + \tfrac{1}{2}C^4 H^4 O^4 + 2CO^2 + 6H.$$
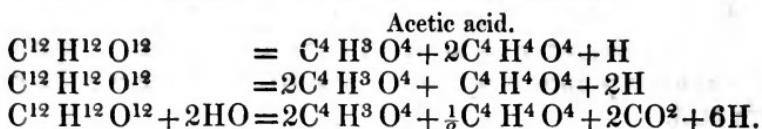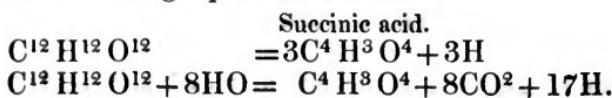
Of these different modes of decomposition, I consider the last as the most probable. At all events I prefer for the present considering it as the true one, since it shows the possibility of all the products of decomposition, except the alcohol, being formed from one equivalent of sugar. It corresponds with the mode of decomposition according to which Liebig supposes malic acid to split up into succinic, acetic, and carbonic acids[*]. Taking this for granted, the following relations will be found to subsist between the products formed in the alcoholic, butyric acid, and succinic acid fermentations of sugar. In all these processes, 1 equiv. of sugar splits up into one or more organic bodies, and one or more inorganic ones. The quantities of carbonic acid formed in the three processes are to one another respectively as 2 : 2 : 1, the quantities of hydrogen as 0 : 2 : 3. In the organic products formed in the three processes (adding together the elements of the succinic and acetic acids of the last process), the numbers of equivalents of carbon, hydrogen and oxygen, are to one another as follows :—in the products of the alcoholic fermentation the C : H as 2 : 3 ; in those of the butyric acid fermentation, as 2 : 2 ; in those of the succinic acid fermentation, as 5 : 4 ; while the numbers of equivalents of oxygen and hydrogen are to one another respectively as 1 : 3, 1 : 2, and 5 : 4. Passing along the series from the products of the alcoholic to those of the succinic acid fermentation, the number of equivalents of hydrogen in the organic substances is found to be constantly on the decrease, as compared with that of the equivalents of carbon and oxygen, while the amount of hydrogen set at liberty increases in the same ratio.

That time may form an important element in all processes of fermentation, and that the degree of rapidity with which such

processes are completed may have a considerable influence on the nature of the products formed, has, I think, been rendered evident by my experiments on the fermentation of rubian. Indeed I would go further, and assert that the difference in the effect produced by the same ferment, under different circumstances, is a direct consequence of the greater or less degree of rapidity in the change which its elements may be undergoing, and of the consequent more or less rapid motion communicated to the elements of other bodies. The very simplest experiments in organic chemistry are sufficient to prove, that a body subjected to a process of rapid decomposition yields very different products to what it does when the same process is slowly conducted.

Now the butyric acid fermentation of sugar differs chiefly from the alcoholic in the length of time which it requires for its completion. The numbers and ratios, which I have here (somewhat hypothetically, perhaps) placed in juxtaposition, seemed to indicate that the fermentation by which succinic acid is formed from sugar is characterized by still greater slowness; and that, by sufficiently retarding the action of ordinary ferments on sugar, we may also by means of these succeed in forming succinic acid in saccharine solutions, perhaps even in considerable quantities.

XXIV. *Further Observations relating to the Theory of Probabilities in reply to* Mr. Wilbraham. *By* George Boole, *LL.D., Professor of Mathematics in Queen's College, Cork**.

SEVERE domestic affliction prevents me from forwarding this month the papers mentioned in the conclusion of my letter just published in the Philosophical Magazine. That letter, and the paper by which it was accompanied, have, I trust, in some degree prepared the way for the more fundamental questions to which I hope shortly to be able to proceed. A careful inquiry into the conditions which a true method must satisfy, may greatly narrow the field of discussion, by entitling us to set aside methods which do not satisfy those conditions, and enabling us to estimate at their just value objections drawn from any assumed advantage of such methods, or, in fact, from assumptions of any kind in the formation of which such conditions have been neglected.

And I conceive that it may thus narrow the field of inquiry in the present instance, if, having already examined Mr. Wilbraham's comparison of the solutions of a certain problem given by Mr. Cayley and myself, I should offer a few remarks, and suggest a question with reference to the method proposed by Mr. Wilbraham himself in the conclusion of his letter (Phil. Mag.

* Communicated by the Author.

vol. vii. p. 476).   Mr. Wilbraham's observations are as follows :—
" If, being in ignorance what system of assumptions ought to
be made to render the problem determinate, we were to wish to
give a definite answer to the problem, it might be in the follow-
ing form : ascertain the chance of the required event happening
on any one system of assumptions, and the chance of that system
representing the true connexion among the simple events, and
multiply the values of these chances together ; the sum of a series
of these products comprising every possible system of assump-
tions would be the true chance of the event.   But Prof. Boole's
method evidently does not attempt to solve any question of this
nature."   Now I make no objection against the truth of the
principle here enunciated, though I may doubt its efficiency.   It
is a principle well known to all who are acquainted with the
elementary treatises on the theory of probabilities.   Moreover, I
think that the principle is *not* opposed to the method which I
have employed, because I have never seen any other method
which leads to " assumptions " (adopting Mr. Wilbraham's lan-
guage) accordant with those conditions which, as we have seen,
must be satisfied.   Leaving such considerations, however, I trust
that the following proposal will not be deemed an unreasonable one.

If Mr. Wilbraham's method is both *correct* and *sufficient,* while
mine is *false,* there must surely be some case in which the two
would lead to different results, and in which, from the comparison
of those results, my own may be proved to be erroneous.   I
would therefore request Mr. Wilbraham to endeavour to furnish
an instance of this kind.   Of course I refer only to problems of
the kind discussed in my work, viz. those in which the data are
the probabilities of events, simple or compound, with or without
information respecting the connexion of such events.   Should
any method, even of limited application, be discovered which
should lead to solutions satisfying the conditions to which I have
referred, and yet different from those furnished by my own
method, which is *not* of limited application, and which *always*
causes those conditions to be satisfied, I should regard it as a
very interesting and remarkable circumstance.   But at present
I am, as I have said, wholly ignorant of the existence of any such
method.

I trust to be able in another month to forward a demonstra-
tion of the general method in probabilities exemplified in the
" Laws of Thought ;" and I am anxious to do this, because the
demonstration may, I think, be presented in a more simple and
satisfactory form than it there possesses, and because an import-
ant addition, (not correction) justified by recent researches, may
be made to the rule there given.

Lincoln, August 5, 1854.

XXV. *On Red and Black Sulphur.* *By* Professor G. MAGNUS[*].

ALTHOUGH the remarkable deportment of sulphur, when exposed to high temperatures, has often been the subject of highly interesting investigations, we are still far from a complete knowledge of all its properties, inasmuch as the different forms which sulphur can assume are more numerous than one would expect. Accidentally I have been led to observe a few allotropic modifications which I believe to be new. In order to describe how these are obtained, it will be necessary to mention several facts already made known to us by the researches of Frankenheim, Marchand and Scheerer, Brame and C. St. Claire-Deville.

Insoluble Yellow Sulphur.

If common sulphur be heated to a temperature of about 360° C. and then suddenly cooled, it is no longer completely soluble in bisulphide of carbon after becoming cool. Also, if sulphur be heated to a lower temperature, and cooled quickly, it is only partially soluble in bisulphide of carbon. The well-known tough, brown sulphur, which Berzelius distinguishes by γ, is not completely soluble. When bisulphide of carbon is poured over this, it immediately becomes white and opake in several places, and gradually so throughout the entire mass. Whether this change is caused by its becoming crystalline, or whether, after the solution of the soluble part, the insoluble remains behind in a porous condition and thereby appears opake, I will not venture to decide.

If common sulphur be allowed to cool slowly instead of quickly, it is, with the exception of the foreign constituents which may be present, completely soluble.

Insoluble sulphur may be kept for weeks and months without its becoming soluble. If melted, however, and allowed to cool slowly, or retained at a temperature not exceeding 130° C., it again becomes soluble. It also reacquires this solubility if heated in a water-bath, but in this case a longer time is necessary.

It is difficult to determine the lowest temperature to which common sulphur must be raised in order to become insoluble by being cooled quickly, because it becomes partially cool in the neck of the vessel out of which it is poured, and because the stratum in immediate contact with the cold water, or the cold body on which it is poured, cools more quickly than the interior mass. Probably this is also the reason why the whole quantity of sulphur which is employed does not become insoluble; for even when a very thin thread of it is poured into cold water, only about 40 per cent. of it is insoluble.

The porous, insoluble sulphur which remains after the solution of the soluble part falls easily to a fine powder, which in the water-bath coalesces to a solid mass.

Flowers of sulphur also contain insoluble sulphur, inasmuch as they are obtained by sudden cooling. But the greater part of them is always soluble; only about a third of their weight is insoluble.

If the sudden cooling of sulphur be repeated, after heating it each time to about 300° C., it has a darker colour after each operation. After six, but often after ten, repeated fusions and sudden coolings it becomes dark brown when cold, and crystallizes, like common melted sulphur, in prisms. In this phase C. Deville has called it *red prismatic sulphur.* I shall call it *repeatedly-melted,* or shorter, *remelted sulphur,* in order to distinguish it from the red sulphur to be afterwards described. The colour of remelted sulphur sometimes changes after a few hours, but generally after some days, to a pale red; at the same time the crystals become opake, and in all probability assume the octahedral form. If the sulphur is fused often enough, however, *e. g.* twenty times, and each time, but particularly the last time, well cooled, it continues amorphous a long time and retains the red-brown colour. When common sulphur is heated only to 160° or 170° C., it does not assume the red-brown colour, even when it has been repeatedly fused at this temperature and well cooled every time. At this temperature some insoluble yellow sulphur is alone formed. In an experiment where the same mass of sulphur was fused ten times at 160° to 170°, and quickly cooled every time, the yellow colour was retained, but more resembling that of amber. Afterwards it was soluble in bisulphide of carbon and colourless; only 9 per cent. of its weight was left behind as insoluble yellow sulphur.

By *remelted* sulphur, in the following pages, is meant sulphur which has been repeatedly fused at *high* temperatures and quickly cooled until it has assumed the red-brown colour.

Such remelted sulphur differs from common in more respects than colour, for in the liquid state at all temperatures, and hence at the point of crystallization, it has a wine-red colour; whereas common sulphur assumes a clear, yellow colour, when by cooling it passes from the thick to the thin liquid state, and it retains this colour until it becomes solid.

When bisulphide of carbon is poured over such remelted sulphur immediately after it is cool and still red-brown, or after it has lain some time and assumed the pale red colour, a red solution is obtained. Its colour will only be weak red or brownish, if, after the last fusion, the sulphur has been suddenly cooled, but deep red if it has been cooled slowly. This difference, which does not appear to have been observed before, is caused by the

peculiar deportment of *black sulphur*, of which we shall after-wards speak.

By evaporating the red solution, or distilling over a part of the bisulphide of carbon, crystals will form in the liquid which remains behind; they have the octahedral form of common sulphur crystallized from bisulphide of carbon.   The first which form are yellow.   After separating these and evaporating the liquid still more, the crystals which then form have always a redder tint.   Ultimately a thick, tough, syrup-like, deep red mass remains behind, from which no more crystals can be obtained.

So far the phænomena are generally known.

Black Sulphur.

When remelted sulphur is treated several times with bisul-phide of carbon, the colour of the solution becomes clearer, until at last a residue, insoluble in bisulphide of carbon, is obtained. This residue, which seldom exceeds one-third of the sulphur em-ployed, falls usually to a brownish powder as long as it contains bisulphide of carbon, but when dried appears almost entirely yellow.

This powder is a mixture of insoluble yellow and of black sulphur.   At the usual temperature it does not change, but at 100° C. it coalesces to a dark brown mass.   By treating this with bisulphide of carbon, a part of it dissolves to a weak reddish liquid; if the insoluble part be again dried, and again exposed for some time to a temperature of 100°, another portion will always be rendered soluble, and the remaining portion always assumes a darker colour; so that when completely dried it appears chocolate-brown.   It now contains more black and less yellow sulphur.   In order to separate the two in this manner, however, care must be taken not to expose them too long to a temperature of 100° C., because at this temperature black sul-phur is also converted into soluble yellow sulphur; the former requiring but a longer time for its conversion than the insoluble yellow.   It is best, therefore, as soon as the mass coalesces into lumps, to take it out of the water-bath and treat it with bisul-phide of carbon.   If solid masses remain behind, they must be powdered, and the powder warmed anew.   The operation is repeated in this manner until the colour of the powder obtained after washing ceases to become darker.

When this powder is fused at 300° C.*, and the fused mass suddenly cooled by pouring it into water, or on a cold body such

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* In order not to exceed the proper temperature, which is of great im-portance in the production of black sulphur, the fusions were always made in metallic baths instead of over a free fire.

as a moistened porcelain plate, in very thin layers, it is, when cool, quite black, soft and viscous, and capable of being drawn out into long thin threads.   It retains its viscidity for many hours, and often days.   It then becomes solid, and has a glassy fracture, without any trace of crystalline structure.

As it is not possible to cool the whole mass suddenly, portions of it always assume a dark brown instead of a black colour; and when the sulphur is preserved for some time, the black parts remain unchanged, whilst the colour of the brown becomes somewhat clearer, and thus gives a lighter tint to the whole mass.   By again heating the so-changed mass to 300° C. and suddenly cooling it, it again appears black; but it now changes more easily than before, inasmuch as it contains more yellow sulphur, the latter being formed at every fusion of black sulphur. The more the black sulphur contains of yellow, the more readily its colour changes.

Pure black sulphur is insoluble in bisulphide of carbon, scarcely soluble in alcohol, æther, benzole, and oil of turpentine, but more soluble in chloroform.   It fuses at nearly the same temperature as common sulphur.   At 180° to 200° C. it becomes a thick liquid.   It can be boiled without changing its colour. When distilled it gives common yellow sulphur.   As black sulphur, however, is obtained by separating the yellow from the repeatedly melted sulphur, it contains the whole of the impurities, insoluble in bisulphide of carbon, which were present in the quantity of common sulphur that was employed in its preparation; and as a great quantity of common sulphur must be used in order to obtain a small quantity of black, these impurities, which in sublimation remain behind, form a considerable part of the black sulphur.   Nevertheless, from red sulphur also we can obtain black which contains no such impurities (see p. 185).   When the latter is sublimated, a scarcely ponderable residue remains, which arises from a little dust.   For, in the many operations to which red sulphur must be subjected, it is scarcely possible to protect it completely from dust.

If *black sulphur* be heated only to 140° or 160°, and retained some time at this temperature, it does not become soft when suddenly cooled, but hardens quickly to a crystalline dark brown mass which dissolves very easily in bisulphide of carbon, and gives a deep red solution.   This brown mass may be called *tempered* sulphur.   In the production of black sulphur some of this is always present, because, in pouring out the mass heated to 300° C., part of it unavoidably sinks to a lower temperature before it can reach the cold water.

From the whole deportment of black sulphur, and particularly from the fact, that when heated to a proper temperature it be-

comes insoluble, and that it may be sublimated without residue, we may conclude that it does not owe its colour to the quantity of carbon it contains. In order to render this certain in another manner, chlorine was conducted over black sulphur which had been prepared from red. After distilling over the chloride of sulphur, a residue remained which amounted to scarcely one-tenth per cent., and this arose evidently from dust which had become mixed with the red sulphur.

The manner in which black sulphur is obtained is very remarkable, for it is apparently formed only by repeated sudden coolings of common sulphur. By heating for a long time at several different temperatures it was not possible to obtain it, even when the temperature of fusion was raised to near the boiling-point of sulphur*. Once or twice only, and that in a manner scarcely explicable, I obtained red-brown sulphur, quite similar to remelted sulphur, at the first cooling after the common sul-phur had been heated for a short time.

It would appear as if, sometimes, the sulphur of commerce, having been several times fused at high temperatures, assumed, on that account, a red-brown colour when first melted. That such sulphur may still appear yellow is evident from the deport-ment of red sulphur (see p. 184).

In all probability *black sulphur* is formed at a certain tempe-rature, and the whole sulphur used might be obtained as black, if it were possible to cool every particle suddenly from that tem-perature down to the point of crystallization. As, however, by pouring into water, or in any other manner, all parts are not cooled equally quickly, only a small portion of black sulphur can in general be obtained.

In order to study the influence of repeated cooling more pre-cisely, a quantity of common sulphur was retained for eight hours at a temperature of 300° C. and then suddenly cooled. It assumed the colour of common sulphur with a slight trace of green. For the sake of comparison, a similar quantity of the same sulphur was, during these eight hours, heated ten times to 300° C., and every time quickly cooled by pouring it on a moist-ened glazed earthenware plate. The one which was fused ten times became more red-brown after each cooling; and although, on the whole, it had been kept a much shorter time at a high temperature it appeared deep red-brown after the last cooling, and deported itself in the same manner as above described in the case of remelted sulphur; that is, it was soluble in, and gave a red solution with bisulphide of carbon, and the insoluble part gave black sulphur. On the contrary, the quantity of sulphur

* In determining these high temperatures, I used the air thermometer which I described in Poggendorff's *Annalen,* vol. lv. p. 1.

which had been uninterruptedly exposed to heat for *eight* hours gave a yellow solution; and the insoluble part, 15·04 per cent., was pure yellow in colour.

To prove that for the formation of black sulphur, not repeated heating, but sudden cooling is necessary, the following experiment was instituted. A quantity of common sulphur was heated to 300° C. in a glass vessel with a narrow neck, which was closed by a cork and placed in a very large sand-bath, where it was completely surrounded by sand. After the sulphur had completely attained the above temperature it was allowed to remain in the large quantity of sand and cool down slowly to the temperature of the room. This operation was repeated ten times with the same sulphur. After this treatment the sulphur gave a colourless solution with bisulphide of carbon; the insoluble residue amounted only to 1·4 per cent. of its weight, but had all the properties of black sulphur. This small quantity of black sulphur was evidently caused by some parts cooling too quickly.

It follows from this, that black sulphur is formed only by sudden cooling.

In the well-known brown, tough, or so-called γ-sulphur, I found that some black sulphur was also present, although it had only been *once* fused: and probably the brown colour and toughness of this sulphur is closely connected with the amount of black sulphur it contains. Black sulphur, like yellow, becomes electric by friction. I must reserve the determination of its specific gravity for another opportunity.

Red Sulphur.

It has been already remarked that *black sulphur*, when tempered, *i. e.* when retained for some time at the temperature of 130° C., dissolves easily in bisulphide of carbon, and gives a deep red solution. When this solution is evaporated, it behaves in exactly the same manner as before described in the case of remelted sulphur; that is to say, it gives a red solution, from which yellow octahedral crystals separate, and a red mother-liquor remains behind, which also permeates, more or less, the yellow crystals. If care be taken to prevent the too quick evaporation of the bisulphide of carbon, by placing the solution under a bell-glass whose rim does not close perfectly, and to take away the crystals which are formed during this slow evaporation as soon as ever a small quantity of the same is visible, then the crystals will appear more and more red in colour; the mother-liquor, however, will at last become thick and tough. If left in this condition it hardens gradually, and finally forms a solid red mass.

To obtain *red sulphur* of a beautiful colour it must be broken into pieces of tolerable size before it is completely hardened. If this is not done it will be long before it becomes solid, and its colour is then less beautiful and more orange. If this method is adopted the sulphur has a cochineal or sealing-wax colour, and forms a porous heap of separate amorphous granules.

When red sulphur has once become solid it is no longer soluble in bisulphide of carbon; it is also little, or not at all, soluble in alcohol, æther, benzole, and oil of turpentine, though rather more so in chloroform.

That this sulphur, although obtained from a solution of bisulphide of carbon by the evaporation of the latter, will not dissolve again in bisulphide of carbon is certainly remarkable, though not without analogy; for silicic acid, after being dried at a high temperature, is no longer soluble. Inasmuch as remelted as well as tempered black sulphur contains yellow as well as red sulphur, it appeared probable that the solubility of red sulphur depends upon the presence of yellow. If red insoluble sulphur, however, be washed with bisulphide of carbon which already contains yellow sulphur in solution, it will be found to be as little soluble in it as in pure bisulphide of carbon. When finely powdered, red insoluble sulphur is mixed with yellow, and the mixture fused, a part of the red becomes soluble, but not more than if the red, *without* the addition of yellow, had been melted at the same temperature. By fusing the red, in fact, a part becomes converted into yellow sulphur; and it would appear as if the intimate mixture of both kinds of sulphur which is thus produced were alone soluble in bisulphide of carbon : not merely because a combination of both has been formed, but because the red sulphur in this mixture is more thoroughly distributed. So distributed, it is soluble; but when its particles lie close together, it ceases to be soluble in bisulphide of carbon. Hence it happens, that when a solution of red sulphur contains yellow, and all the bisulphide of carbon has been withdrawn before the yellow sulphur has had time to crystallize, the solid mass is partly soluble again, and gives a red solution. We may therefore assert that all solutions of red sulphur are mixtures of red and yellow. If such a solution contains little red and much yellow sulphur, then on evaporating, crystals are formed which, if apparently red, are yet soluble; they evidently owe their colour to the mother-liquor which they enclose. If the solution contains much red and little yellow sulphur, the crystals are soluble with the exception of a residue of red sulphur. This residue, however, is not so red as that which is obtained after all the yellow sulphur has been separated by crystallization; it is more yellow-red, and sometimes even yellow. This difference in colour evi-

dently depends upon the state of aggregation, for even red sulphur, when finely powdered, appears yellow-red.

But this yellow-red powder, even should it appear quite yellow, may be easily distinguished from the yellow insoluble sulphur by fusing at 110° to 130° C., when, on cooling, it will form quite red masses; or by fusing at 300° C. and cooling quickly, when it will be converted into black sulphur; in short, it will possess all the properties of red sulphur.

Insoluble red sulphur, whether it be quite red or yellowish, remains unchanged on keeping. At least I have preserved specimens of it, of several different tints, for many months without observing any change. A mixture of much red and little yellow sulphur, however, changes its colour gradually from red to yellow.

Even in solution red sulphur suffers this change. A deep red solution was preserved in a well-closed flask; its redness continually decreased in intensity until, in two months, it appeared orange. Whether this change was caused by the heat of the sun, to which the solution was sometimes exposed, or whether it proceeded in darkness also, I must leave undecided.

As Pasteur and Deville have already shown*, *prismatic* crystals are formed in a concentrated solution of red sulphur with little yellow, which at first are red, but after some time yellow and opake. I have found that of these, many are only partially soluble in bisulphide of carbon. The insoluble yellow residue often retains a prismatic form, but deports itself like red sulphur.

But, inasmuch as red sulphur possesses no crystalline structure, the prismatic form evidently belongs to the yellow sulphur, which, as Deville has already shown†, can assume that form at the usual temperature if crystallized from certain solvents, and which, when crystallized from bisulphide of carbon, apparently assumes that form only because red sulphur is at the same time present in the solution. It is certainly remarkable that a body at the same temperature crystallizes in two different systems, though we know that with certain salts, *e. g.* alum, certain surfaces are principally formed when the solution is acid, and others when it is basic. But when, by the presence of a foreign substance which does not at all enter into the crystals, the relations of cohesion become altogether changed, it is possible that this change may be so great that the crystals form themselves according to a quite different system. This is especially conceivable in sulphur, which in every case passes so easily from one system to another.

When red sulphur is exposed for some time to a temperature of 100° C., it becomes converted into soluble yellow sulphur.

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* *Comptes Rendus de l'Academie des Sciences,* vol. xxvi. pp. 48, 117.
† Ibid. vol. xxxiv. p. 534.

If in a state of fine powder, it coalesces when heated; if employed in larger pieces, it retains its form unchanged.

If pure red sulphur be fused at 110° to 130° C., and suddenly cooled as soon as it has become liquid, a red molten msss will be obtained which shows a crystalline structure at the surface. By treating this with bisulphide of carbon, a portion of it dissolves and gives a reddish solution; the greater part is insoluble.

Red sulphur, when fused at 130° to 150° C., is soluble, and gives a red solution.

When fused at 300° C. and suddenly cooled, black sulphur is obtained which possesses all the properties of the above-mentioned black sulphur, and, like the latter, remains for a long time quite soft and ductile.

Black sulphur, when prepared in this manner, always contains small quantities of yellow sulphur, inasmuch as the heating to 300°, and the subsequent cooling, cannot be momentarily effected; but with this exception it is pure, whilst the black sulphur prepared according to the method above mentioned, contains all the impurities, insoluble in bisulphide of carbon, which were present in the yellow sulphur used in its preparation.

Red sulphur, when sublimated, gives yellow, without production of residue. In a similar manner to that already mentioned, black sulphur, when sublimated, also gives yellow.

This easy conversion of red as well as of black sulphur into soluble yellow sulphur at high temperatures, is the reason why both can be obtained only in so small quantities.

Black sulphur can only be obtained from red by heating the latter to 300° C. and then suddenly cooling it. Nevertheless the deportment and external appearance of both are so essentially different, that it appears perfectly justifiable to consider them as two different modifications. Soluble red sulphur differs as much from insoluble red and from black as these do from one another, hence it is advisable to consider this also as a particular modification; and we have as great cause for so doing, as for considering soluble and insoluble silicic acid as two different modifications.

According to the above, the following are the results:—

1. When properly heated sulphur is suddenly cooled, a part of the same is converted into insoluble yellow sulphur. One-third of the weight of flowers of sulphur consists of such insoluble sulphur.

2. By repeated fusings at a high temperature, and sudden coolings, sulphur assumes a red-brown colour. In this state Deville calls it red sulphur. It would be more convenient to call it *remelted* sulphur, in order to distinguish it from actual

red sulphur. About a third of the same is soluble in bisulphide of carbon. This third has a yellow colour, but consists partly of yellow and partly of insoluble black sulphur.

. 3. The above insoluble residue, when warmed in the water-bath, becomes soft and greasy; and after cooling, one portion (the yellow) is soluble, inasmuch as this, at 100° C., is more easily converted into the soluble modification than the black.

By repeatedly treating the insoluble residue at 100° C., the residual powder assumes a chocolate-brown colour.

4. This powder, when fused at 300° C. and suddenly cooled, gives a perfectly black, greasy, ductile mass, which requires some time to solidify, and has then a glassy appearance. This *black sulphur* does not dissolve, or does so very slightly, in bisulphide of carbon, alcohol, æther, benzole, oil of turpentine, and chloroform.

5. *Black sulphur,* when kept for some time at a temperature of 100° C., dissolves in bisulphide of carbon. The solution contains much yellow, together with a little red sulphur.

6. If *black sulphur* be exposed for a short time only to a temperature of 130° to 150° C. it will, when cool, have a brown colour and crystalline structure. It is now soluble in bichloride of carbon. The solution contains much red and little yellow sulphur.

7. By evaporating the solution slowly, crystals of yellow sulphur are obtained which appear the more red the more concentrated the solution; at last a mother-liquor is left behind, which after some time forms a solid red mass.

8. This hardened mass is not again soluble in bisulphide of carbon. By proper treatment it is granulous, and has a cochineal-red colour.

9. This *red sulphur,* when exposed to a temperature of 130° to 150° C., fuses to a red mass of crystalline structure, of which a part is soluble in bisulphide of carbon.

10. *Red sulphur,* when heated to 300° C. and suddenly cooled, gives *black sulphur.*

Accordingly, the following are the several modifications of sulphur :—

                1. Soluble prismatic yellow,
                2. Soluble octahedral yellow,
                3. Insoluble yellow,
                4. Insoluble red,
                5. Soluble red,
                6. Black sulphur.

The remelted and suddenly-cooled sulphur contains soluble yellow, insoluble yellow, black and soluble red sulphur.

XXVI. *On some new Theorems in Arithmetic.*
By J. J. Sylvester, *F.R.S.**

L ET $S_i(a, b, c, \ldots k, l)$ denote, as is not unusual, the complete sum of the products of the elements (n in number) $a, b, c, \ldots k, l$, combined in every possible way i and i together. Let $\ddot{S}_i(a, b, c, \ldots k, l)$ denote the sum of the products of the same elements combined i and i together, but so that all combinations are excluded in which any two consecutive elements as a and b, or b and c, $\ldots$ or k and l, appear simultaneously. S_i may be termed a complete sum of ith products, and $\ddot{S}_i$ a sum of products of *anakolouthic* elements, or briefly an anakolouthic sum of ith products. If we expand the continued fraction

$$\frac{1}{\rho+} \frac{\dot{a}}{\rho+} \frac{b}{\rho+} \cdots \frac{k}{\rho+} \frac{l}{\rho},$$

it will be easily found to take the form

$$\frac{\rho^{n-1} + \ddot{S}'_1 \rho^{n-3} + \ddot{S}'_2 \rho^{n-5} + \&c.}{\rho^n + \ddot{S}_1 \rho^{n-2} + \ddot{S}_2 \rho^{n-4} + \&c.};$$

where $\ddot{S}'_i$ is intended to denote the anakolouthic sum of the ith products of $b, c, \ldots l$, and $\ddot{S}_i$ the anakolouthic sum of the ith products of $a, b, c, \ldots l$.

It is this fact, and the close relation of reciprocity in which the generating continued fraction for anakolouthic sums stands to ordinary continued fractions (a reciprocity which becomes more apparent when ρ is made unity), which gives a peculiar importance to the theory of anakolouthic sums of the kind denoted by $\ddot{S}$; otherwise we might be tempted to embark upon a premature generalization, extending the force of the term anakolouthic so as to denote by $\dddot{S}$ a sum of products in which no *three* consecutive elements came together, $\ddddot{S}$ a sum of products in which no four consecutive elements came together, and so on; these more general forms of anakolouthic sums may hereafter merit and reward attention, but my present business will be exclusively with a statement of some remarkable properties which have accidentally fallen under my observation, of anakolouthic sums of the kind first mentioned, and referring to elements formed in a manner presently to be explained, from the natural progression of numbers. In order to familiarize the reader with the construction of nakolouthic series, I subjoin the following examples :—

$$\ddot{S}_1(abcde) = a + b + c + d + e$$
$$\ddot{S}_2(abcde) = ac + ad + ae + bd + be + ce$$
$$\ddot{S}_3(abcde) = ace$$
$$\ddot{S}_4(abcde) = 0$$
$$\ddot{S}_4(abcdef) = 0$$
$$\ddot{S}_4(abcdefg) = aceg$$
$$\ddot{S}_4(abcdefgh) = aceg + aceh + bdfh.$$

First Theorem. Let n be any odd number; form the elements $\left(\text{in number } \dfrac{n-1}{2}\right)$

$$(n-1),\ 2(n-2),\ 3(n-3) \ldots \frac{n-1}{2}\cdot\frac{n+3}{2};$$

the anakolouthic sum of the ith products of these elements is equal to the ith power of negative unity into the complete sum of the $2i$th products of the elements n, $(n-2)$, $(n-4)$, $\ldots \pm 1$. Thus suppose $n = 7$, the elements for the anakolouthic sums will be

$$7,\ 12,\ 15;$$

and for the complete sums,

$$7,\ -5,\ 3,\ -1;$$

and we find

$$\ddot{S}_1(7,\ 12,\ 15) = 7 + 12 + 15 = 34$$
$$\ddot{S}_2(7,\ -5,\ 3,\ -1) = -7\times 3 - 5\times 2 - 3 = -34$$
$$\ddot{S}_2(7,\ 12,\ 15) = 7.15 = 105\quad S_4(7,-5,3,-1) = 1.3.5.7 = 105.$$

Or, again, if $n = 9$, the one set of elements will be

$$9,\ 16,\ 21,\ 24,$$

and the other set

$$9,\ -7,\ 5,\ -3,\ 1;$$

and we have

$$-(9+16+21+24) = 70 = 9\times(-4) + 7(-3) + 5(-2) + 3(-1)$$
$$9.21 + 9.24 + 16.24 = 789 = 9.7.5.3 + 9.7.3.1 - 9.7.5.1$$
$$-9.5.3.1 + 7.5.3.1.$$

Second Theorem. Take away the last element belonging to the anakolouthic groups above written, so as to reduce the elements to the following sequence:

$$1 - n,\ 2(n-2),\ 3(n-3) \ldots \frac{n-3}{2}\cdot\frac{n+5}{2}.$$

$\dfrac{n+1}{2}$ times the anakolouthic sum of ith products of this sequence

will be equal to $(-1)^i$ multiplied by the complete sum of the $(2i+1)$th products of the series n, $-(n-2)$, $(n-4)$, ... ± 1. Thus if $n=9$, the two series of elements are respectively

$$9, 16, 21; \quad 9, -7, 5, -3, 1;$$

and we find

$$5 \times 1 = 9 - 7 + 5 - 3 + 1$$

$$5 \times (9+16+21) = 230 = 9.7.5 - 9.7.3 + 9.7.1 + 9.5.3$$
$$-9.5.1 + 9.3.1 - 7.5.3 + 7.5.1 - 7.3.1 + 5.3.1$$

$$5 \times (9 \times 21) = 9 \times 7 \times 5 \times 3 \times 1.$$

I now pass on to the cases where n is an even number.

Third Theorem. Let n be of the form $4m+k$, where k is zero or 2; construct the sequence

$$1.n, \quad 2(n-1), \quad 3(n-2) \ldots \left(\frac{n}{2}-1\right)\left(\frac{n}{2}+2\right);$$

the ith anakolouthic series of products formed out of these elements is equal to the ith complete series of products formed out of the elements $(n-2)^2$, $(n-6)^2$, ... k^2.

Ex. Let $n=10$, the two sequences will be

$$10, 18, 24, 28,$$
$$64, 16, 0,$$

and we have

$$10+18+24+28 = 80 = 64+16$$

$$10 \times 24 + 10 \times 28 + 18 \times 28 = 1024 = 16 \times 64.$$

So, if $n=12$, the two sequences will be

$$12, 22, 30, 36, 40$$
$$100, 36, 4;$$

and we have

$$12+22+30+36+40 = 140 = 100+36+4$$

$$12 \times (30+36+40) + 22 \times (36+40) + 30 \times 40$$
$$= 4144 = 100 \times 36 + 100 \times 4 + 36 \times 4$$

$$12 \times 30 \times 40 = 4 \times 36 \times 100.$$

Fourth Theorem. If n be any even number, and we form the three sequences

$$1.n, \quad 2(n-1), \quad 3(n-2) \ldots \frac{n}{2}\left(\frac{n}{2}+1\right)$$

$$1.(n+2), \quad 2(n+1), \quad 3(n) \ldots \frac{n}{2}\left(\frac{n}{2}+3\right)$$

$$1.n, \quad 2(n-1), \quad 3(n-2) \ldots \left(\frac{n}{2}-2\right)\left(\frac{n}{2}+3\right),$$

the ith anakolouthic sum in respect to the second sequence less the ith anakolouthic sum in respect to the first sequence is equal to $\frac{n}{2}\left(\frac{n}{2}+1\right)$ into the $(i-1)$th anakolouthic sum in respect to the third sequence.

Ex. Take the three sequences

$$1.10 \quad 2.9 \quad 3.8 \quad 4.7 \quad 5.6$$
$$1.12 \quad 2.11 \quad 3.10 \quad 4.9 \quad 5.8$$
$$1.10 \quad 2.9 \quad 3.8.$$

These, written out with simple elements, are as follows :—

$$10 \quad 18 \quad 24 \quad 28 \quad 30$$
$$12 \quad 22 \quad 30 \quad 36 \quad 40$$
$$10 \quad 18 \quad 24;$$

and we have

$$(12+22+30+36+40)-(10+18+24+28+30)=30\times 1$$
$$\{12\times(30+36+40)+22\times(36+40)+30\times 40\}$$
$$-\{10(24+28+30)+18(28+30)+24\times 30\}$$
$$=4144-2584=1560=30\times(10+18+24)$$
$$12.30.40-10.24.30=14400-7200=7200$$
$$=30\times(10\times 24).$$

These four theorems are only particular cases of one much more general relating to a determinant, to which I was led by my method of integrating the system of two partial differential equations to the general invariant of a function or system of functions of two variables. In like manner the integration of the system of t partial differential equations to the general invariant of a function or system of functions of t variables conducts to a determinant[*], of which *à priori* we know the constitution, and which will (save as to the periodic occurrence of a single factor λ) resolve itself into factors of the form $\lambda^t \pm m^t$, m being an integer, and thus promises to lay open a road to the discovery of a new genus of theorems relating to the powers of the natural progression of integer numbers, destined apparently to occupy a sort of neutral ground between the formal and quantitative arithmetics.

25 Lincoln's Inn Fields,
   July 11, 1854.

[*] The integration of this system of equations always depends essentially upon the integration of one homogeneous equation which is doubly linear, *i. e.* of the first degree in the variables, and also of the first degree in respect to the order of the differentiations; such an equation can always be integrated, and the integral will depend upon the solution of an algebraical equation expressed by equating a certain determinant to zero.

XXVII. *On the power of conducting Electricity assumed by Insulators at High Temperatures.* *By* W. Beetz[*].

THE accounts given by several physicists concerning the property which many substances possess of passing from the condition of insulators to that of conductors, by increase of temperature, have reference to bodies so heterogeneous that it appears almost impossible to reduce the phænomena to one cause. Davy's experiments[†] prove that the conducting power of metallic conductors is decreased by heating them; whilst, according to Ohm's observations[‡], a decrease of resistance takes place in electrolytic conductors under similar circumstances. Hence the passage of a body by means of heat from the condition of a non-conductor, *i. e.* of a very bad conductor, to that of a conductor, or better conductor, can be very well explained if the body be a compound one, but not at all if it be an elementary one. In the following pages I have described the experiments by means of which I hope to have secured a common point of view for all these several statements.

Of the elementary bodies which are liquid at the usual temperatures, the one, quicksilver, deports itself in every respect as a conductor of the first class, and hence, as shown by E. Becquerel's[§] measurements, its conducting power is decreased by heating. The other, bromine, according to Balard's experiments[||], is a non-conductor of galvanic electricity; a stratum from 3 to 4 lines thick, when introduced into the circuit of a battery, caused every perceptible action in a decomposition apparatus to cease. De la Rive[¶] obtained the same result, and mentions at the same time that, according to a verbal communication of Faraday's, liquid chlorine is a non-conductor, and is not acted upon by a battery. Solly[**] found that bromine did not conduct, but was at first of opinion that chlorine was a conductor; after carefully washing the tube in which it was contained, however, he found it to be a perfect non-conductor. When sufficiently well freed from admixed water, I have also found bromine to be a perfect non-conductor of galvanic electricity. Those elements which are made liquid by fusion are generally *metallic* in the solid state, and consequently lose conducting power when heated. Of the non-metallic bodies,

[*] Communicated to the Academy of Sciences, Berlin, June 13, 1854.
[†] Phil. Trans. 1821, p. 431.
[‡] Pogg. *Ann.* vol. lxiii. p. 403.
[§] *Ann. de Chim. et de Phys.* 3 sér. vol. xvii. p. 234.
[||] Ibid, vol. xxxii. p. 345; Pogg. *Ann.* vol. viii. p. 123.
[¶] *Ann. de Chim. et de Phys.* vol. xxxv. p. 160; Pogg. *Ann.* vol. x. p. 307.
[**] Phil. Mag. S. 3. vol. viii. p. 130; Pogg. *Ann.* vol. xxxvii. p. 420.

Faraday* has shown that liquid sulphur and phosphorus do not conduct; hence the statement of J. Inglis† that iodine conducts when fused is quite unexpected.   In this case the substance to be tested was contained in a glass tube sealed at both ends, into which two platinum wires were conducted.   Solly ‡ contradicted this experiment: he could neither detect the slightest trace by Wollaston's method (in which the body to be investigated is introduced into a circuit, both of whose extremities consist of different metals which can be placed on the tongue), nor could he observe any decomposition in a solution of iodide of potassium, by means of a battery of 60 elements, when the circuit was interrupted by a stratum of melted iodine $\frac{1}{20}$th of an inch thick.

Notwithstanding this, Palmieri §, relying upon the experiments of Inglis, and without mentioning those of Solly, has lately constructed batteries from three elementary bodies, *e. g.* from two metals and iodine, wherein the latter takes the place of the electrolyte.   If the statement concerning the iodine battery was not so simple as to preclude the possibility of error, I should pay no regard to this investigation, seeing that the same physicist constructs a battery equally well from three metals, one of which is mercury, whose action can evidently be ascribed to no other cause than to the moisture adhering to the mercury.   A glass flask which contained somewhat more than one ounce of iodine was closed with a cork, the iodine was then fused, and air admitted several times during the process by removing the cork, after which two plates of platinum and iron, connected with a delicate galvanometer, were immersed in the liquid mass.   The galvanometer at once announced a current traversing the wire from the platinum to the iron.   No perceptible residue was at first obtained when the iodine which had been used was evaporated on a platinum plate; after repeating the experiment several times a small residue containing traces of iron was obtained. The action was still stronger when iodine was fused in a platinum crucible and an iron plate dipped into it, the crucible and plate being both connected with the galvanometer.

In order first to come to a decision as to the fact of the conducting power of liquid iodine, I made the following experiment. Two platinum wires connected with a galvanometer were immersed in a porcelain dish containing iodine, and the dish heated. As the iodine began to fuse, a deviation of the astatic system, amounting to about 15°, was observed; when the wires were immersed after the iodine had been fused, the deviation was

* Experimental Researches, par. 405.
† *Bibl. Univ. de Gen. Nouv.* sér. 1. p. 167; Phil. Mag. S. 3. vol. vii. p. 441.
‡ *Ann. de Chim. et de Phys.* vol. xxxv. p. 160; Pogg. *Ann.* vol. x. p. 307.
§ *Rendic. della R. Acc. de Nap.* vol. ix. p. 161.

somewhat less, probably because the wires became covered with a stratum of solidifying iodine. Conduction, therefore, although very little, certainly took place. In order to ascertain whether this conduction was metallic or electrolytic, the wires were so arranged as to be in connexion with the battery in one position, and with the galvanometer in another. After the current had passed for some time from one wire through the iodine to the other, the two were connected with the galvanometer, when a deviation of the needle, amounting to between 1 and 4 degrees, ensued, and always in the same direction as must have occurred in any ordinary galvanic decomposition. The conduction, therefore, was electrolytic, or at any rate partly so, and could not on that account be peculiar to the iodine itself. In order to ascertain whether a rapid solution of platinum in iodine furnished the small quantity of electrolyte which was here decomposed, the platinum wires were replaced by pieces of graphite, a substance employed by Faraday* as electrodes in the electrolysis of chlorides and iodides, because it was not acted upon by these bodies; conduction, however, still took place, which must have been caused by the slight impurity of the iodine. After twice subliming about an ounce of this substance at a gentle heat, so that each time only about half the quantity used was sublimated, the experiments were repeated with the thus purified iodine. The needle still deviated, but at most to 5 degrees, when the fused iodine was introduced into the circuit which contained the galvanometer, and still the electrodes became charged. At length I sought to destroy the impurity in an electrolytic manner by introducing the iodine into a glass tube into which two platinum wires were led, and which was afterwards completely sealed up; the deviation decreased a little, but afterwards remained pretty constant, although a zinc and iron battery of six elements acted on it for three hours. The platinum wires remained polarized, but the conduction electrolytic to the last. On opening the glass and testing the iodine by evaporation, a small residue was obtained. Hence I think we may with certainty conclude, that the very small conducting power of purified iodine is due to the admixture of a small quantity of some volatile electrolyte (hydriodic acid?); but that during the continued action of the current the electrodes begin to be acted upon, and thus new, though very small, quantities of an electrolyte become mixed with the iodine, whilst this substance itself is a non-conductor. Hence there remains no elementary body which, when converted into the liquid state, becomes a better conductor.

Faraday's† beautiful experiments have already thrown much

* Experimental Researches, par. 794.   † Ibid. Ser. V. and VII.

light on the compound insulators, which by fusion become conductors; to a great extent they form but a higher class in the series of bodies which conduct at ordinary temperatures, and which at lower temperatures, on solidification, become non-conductors. Of these, water is an example. Franklin* and Achard† have shown that in its solid state, as ice, it insulates frictional electricity; and since then, Faraday‡ found that its insulating action is still more decided towards galvanic electricity. Faraday, however, found some exceptions to the general rule, that the compound bodies, which by fusing have become conductors, still follow the law of fixed electrolytic action. In the several series of his Researches he has expressed several views of the deportment of these exceptional bodies, the most important of which are sulphuret of silver§, periodide of mercury‖, and fluoride of lead¶; bodies which, on increasing their temperature, become conductors, but which show no traces of decomposition. Hittorff's** experiments on the first of these substances are so complete, that it is not necessary for me to enter on the same. Sulphuret of silver deports itself as an actual electrolyte, but at increased temperatures the separated particles of silver combine and form a metallic thread or covering, which connects both ends, and causes the whole conductor to become a metallic one. When the body cools, conduction ceases, owing to the rupture of this thread. To sulphuret of silver Hittorff adds another substance, sulphuret of copper, whose apparent metallic conduction he also reduces to an electrolytic one.

When publishing his Experimental Researches, Faraday, in a note to paragraph 448, concerning periodide of mercury, remarks, " It is just possible that this case may, by more delicate experiment, hereafter disappear." In paragraph 691 the case is, in fact, more carefully considered. "The case is an exception, and I think the only one, to the statement, that all bodies subject to the law of liquido-conduction are decomposable. I incline, however, to believe that a portion of protiodide of mercury is retained dissolved in the periodide, and that to its slow decomposition the feeble conducting power is due. Periodide would be formed, as a secondary result, at the *anode*; and the mercury at the *cathode* would also form, as a secondary result, protiodide. Both these bodies would mingle with the fluid mass, and thus no final separation appear, notwithstanding the con-

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* Experiments and Observations, p. 36.
† *Chemisch physische Schriften.* Berl. 1780, p. 11.
‡ Experimental Researches, par. 384.
§ Ibid. pars. 433, 437, 438, 1340.
‖ Ibid. pars. 414, 448, 691, 1341.
¶ Ibid. par. 1340.
** Pogg. *Ann.* vol. lxxxiv. p. 1.

tinued decomposition." Lastly, however, in par. 1341, he says, "I have now very little doubt that periodide of mercury is a case of the same kind" (*i. e.* a case in which a body, by fusion, assumes a conducting power of its own without decomposition).

In order to purify periodide of mercury, it was also twice sublimated and then placed for experiment in a glass tube, into one end of which a platinum wire had been before fused. The periodide of mercury was then retained for a long time in a state of fusion, and almost of ebullition, in order to dispel any moisture which might possibly have been enclosed; the other end of the tube was then closed and a second platinum wire fused into it. Besides this apparatus, a galvanometer with a simple needle, and a silver voltameter were introduced into the circuit; and lastly, the two platinum wires could be connected at any moment by means of a commutator, with a second galvanometer containing an astatic system of needles. The glass tube filled with periodide of mercury was heated in a sand-bath. The salt began to conduct at about 110° before it became liquid; the commencement of conduction was simultaneous with that of the yellow colour which periodide always assumes immediately before fusing. From the first moment of conduction, a polarization of the platinum wires was observed on turning the commutator, thus disproving the hypothesis that this yellow periodide possesses conducting power without decomposition. The glass tube was now more intensely heated, the galvanometer needle retained a tolerably fixed position; the experiment was concluded in about an hour, and the glass tube broken open. Neither iodine nor mercury could be detected on either wire by any reagent, although 0·005 grm. of silver were precipitated in the voltameter, proving that about the same quantity of mercury and 0·006 grm. of iodine must have been separated. Nevertheless the opinion which Faraday last expressed, that periodide of mercury possesses a peculiar power of conducting, could not be maintained, because the polarization proved the existence of a permanent electrolysis. I hoped, therefore, by a continued action to remove an electrolytic impurity which was perhaps mixed with the periodide; and to do this, I conducted a current from a zinc and iron battery of six elements for fourteen hours through a new apparatus of the same kind (containing about 8 grms. of iodine), taking care to preserve the mass in a liquid state. The galvanometer needle soon assumed a tolerably permanent position, from which it appeared to deviate only in consequence of changes of temperature. At the end of this time 0·162 grm. of silver were precipitated in the voltameter; the tube was opened, and even now no mercury was perceptible on the negative wire, but in the neighbourhood of the positive wire the mass was coloured black; and

when pieces of the same were thrown on paper saturated with starch, the violet colour which the latter assumed was as intense as that produced by pure iodine itself. Inasmuch as the deviation of the galvanometer needle remained constant, the electrolysis cannot be ascribed to any impurity, or at most it may be due to an impurity which continually forms itself anew in the mass itself, *i. e.* to protiodide of mercury, as Faraday formerly supposed. As, however, according to my experiments, free iodine was liberated, all the protiodide which may have been present in the vicinity of the positive wire must first have been converted into periodide; and if this had not itself conducted, the current would have been interrupted. In the first experiments, which were of short duration, this formation of periodide was evidently not complete. Once complete, however, the liberation of iodine proceeds rapidly, whilst protiodide is formed at the negative pole; hence periodide must, in fact, be an electrolyte. A quantitative determination of the separated iodine is here quite impossible, but apparently it was less than 0·190 grm., which, according to the voltameter, must have been separated; on this account I conjecture that a part of this iodine must have again combined with the protiodide which was contained in mechanical mixture. Accordingly a very small tube was filled with 0·182 grm. of periodide of mercury. After ten hours' action 0·098 grm. of silver were precipitated, according to which 0·114 grm. of iodine should have been set free. But the periodide only contained 0·101 grm. of iodine, and yet the deviation of the galvanometer needle had been almost constant, so that we are compelled to conclude that a recombination took place. This recombination also showed itself externally, for the limit of the black-coloured part lay far nearer to the positive pole than in the former experiment, although the quantity of iodine to be separated was not then double as great, and yet the breadth of the tube was much greater.

A second circumstance may also diminish the liberation of iodine. It is well known that the protiodide, by heating, is decomposed into mercury and periodide. When a galvanic current was conducted through heated protiodide, mercury was separated strongly at the negative pole; but within the mass, also, particles of mercury appeared, though certainly to a much less degree. This must also be the case with the protiodide which arises from the decomposition of the periodide, and is held by it in solution; branch currents may therefore be produced, for whose formation the existence of a thread of mercury joining the poles is not at all necessary. Only as much iodine will in every case be separated at the positive pole as corresponds to the branch current which passes immediately through the molten mass, and not

through the mercury; whilst every electrolysis which takes place between two particles of mercury within the liquid conductor decomposes as much periodide on the one hand as it forms on the other, so that at most the position of the particles of mercury will be changed.

Of fluoride of lead, Faraday speaks still more definitely in par. 1340: " When a piece of that substance, which had been fused and cooled, was introduced into the circuit of a voltaic battery, it stopped the current. Being heated, it acquired conducting powers before it was visibly red-hot in daylight, and even sparks could be taken against it whilst still solid. The current alone then raised its temperature until it fused, after which it seemed to conduct as well as the metallic vessel containing it. . . . . During all the time there was scarcely a trace of decomposing action of the fluoride, and what did occur seemed referable to the air and moisture of the atmosphere, and not to electrolytic action." Fremy*, on the contrary, counts fluoride of lead amongst the decomposable salts.

At several different times I employed the greatest care in preparing fluoride of lead from fluoride of potassium and acetate or nitrate of lead. The electrical properties I found were almost exactly what Faraday had stated them to be, although the substance conducted far worse than metals, but far better than periodide of mercury. As soon as conduction commenced, the platinum plates in contact with the mass became polarized. As the mass began to fuse, a colourless gas escaped in small, but quite perceptible, bubbles at the positive pole, around which the liquid had a brown colour. A gray body collected around the negative pole. The electrodes were examined after the process had continued an hour, during which time the galvanometer needle soon went back to a position of rest. The positive electrode was blackened and perceptibly acted upon. The black colour could not be diminished by boiling with fuming nitric acid or with potash. The negative electrode had swelled out to a porous mass, in which I detected an alloy of lead and platinum; accordingly, the gas at the positive pole must have been fluorine, so that the decomposition was quite analogous to that of chloride of lead. In another experiment I found that 0·014 grm. of silver had been precipitated in the voltameter, whereas the negative electrode had increased 0·019 grm. (instead of 0·013); this may have arisen from an imperfect separation of the alloy from the surrounding liquid, in consequence of the brittle, sponge-like nature of the former. As, according to these experiments, fluoride of lead followed the laws of electrolytic action almost exactly, I began to fear that some other compound of

* *Comptes Rendus*, vol. xxxviii. p. 393; *Journ. für prakt. Chem.* vol. lxii. p. 65.

lead might have been admixed, and thus have been electrolysed; by no reagent, however, could any such compound be detected; and on allowing the current to pass through the fluoride of lead for six hours, the deflection of the galvanometer needle remained constant during the whole period. The experiment was repeated with a specimen of fluoride of lead which M. H. Rose had kindly given me, and in which I also failed to detect any foreign substance; exactly the same results were obtained, so that fluoride of lead may decidedly be considered as an electrolyte.

Experiments on substances containing silicic acid, showing how their power of conducting commences when heated, have long since been made known. After Cavendish* had demonstrated this conducting power for frictional electricity in glass, and Delaval† in Portland and several other stones, Aldini‡ was able to produce contractions in a prepared frog by connecting the two metals which were placed on muscle and nerve by means of a piece of glass fused before the blowpipe. And although Ritter§ claimed an insulating power for red-hot glass, yet Pfaff‖ received continuous shocks when with both hands he completed the circuit of a voltaic battery into which a glass tube heated to redness and almost to fusion was inserted. Later, when he corroborated these statements, Ritter¶ proposed the following questions:—" Of what nature is the conducting power of those individuals of this class of bodies that ultimately conduct? Do they conduct in the same manner as water, or in the same manner as metals, *i. e.* with or without decomposition, or more definitely in Volta's language, as conductors of the first or of the second class?" In the foregoing case this last question was exactly the one under discussion. For my first experiments I selected Fuch's water-glass, which, on account of its simple composition, may be considered as a type of all the other silicic-acid compounds. This glass was prepared without any addition of carbon, and was almost colourless or white. A small narrow bar of it was placed on two strips of platinum and strongly heated until it fused fast to them, it was then allowed to cool again. When cold, it conducted the current of a battery connected with the two pieces of platinum; when gently heated or dried over sulphuric acid, it was a perfect insulator. When strongly heated, but not to fusion, it became a good conductor, and from the first motion of the astatic system in the galvanometer which was introduced into the circuit, it was evident that the platinum plates

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* Franklin's *Experiments and Observations.* London, 1774, p. 411.
† Priestley, *Gesch. d. Electr. Deutsch v. Krünitz,* 1772, p. 150.
‡ Aldini, *Versuche, übers. v. Martens,* 1804, vol. ii. p. 76.
§ Gilbert's *Annalen,* vol. vi. p. 471.
‖ Ibid. vol. vii. p. 249.   ¶ Ibid. vol. ix. p. 290.

were polarized. The same experiment was made with frictional electricity. Here also a polarization immediately ensued, which was similar to the one observed by Henrici* when passing the electricity of a machine through water. In order to determine whether to ascribe the counter currents which were formed to an electrolytic action, or to a deportment similar to that shown by imperfect conductors, a piece of water-glass, after the current of a battery had been passed through it for some time, was broken loose from its supports. Before, the glass had a weak alkaline reaction ; this reaction was now no longer manifest at the end of the bar which had rested on the positive plate, but at the other it was strong. This substance, therefore, had deported itself, even before fusion, exactly as an electrolyte would have done.

After this I proceeded to experiment upon common glass : the form generally employed was that of a thin tube, into which two platinum wires were fused so as not to touch one another, in order to avoid every external influence. When a gentle heat was required, the sand-bath was used; when a strong one, the immediate action of a flame. The sand-bath could be well employed in all experiments; for the sand, even when strongly heated, conducted so badly, that it was only able to discharge a charged electroscope very gradually. The several kinds of glass which were employed began to conduct the galvanic current when between 200° and 220° C. (Becquerel† found this to occur only at 300°), when the wires became strongly polarized. In order to find whether an electrolytic action was also present in this case, the experiment was made in the same manner as with the water-glass ; a small glass bar was fused fast to two strips of platinum, and broken off after it had been exposed for two hours to the action of a current from a battery of six or ten elements ; the end which had lain on the negative plate was rubbed smooth and moistened with water : in two out of seven experiments a weak basic reaction was detected. As this experiment was not decisive, the glass bar, which had been thus fused to the two plates and exposed to the action of the current, was broken off and replaced by a fresh one after the battery had been withdrawn from the circuit. A polarization-current was always obtained when this bar was heated, thus proving that chemically-opposite changed substances had remained behind on the plates. Frictional electricity also presented the same phænomena of polarization, in a much less degree it is true, but still always in the proper direction. In these experiments, however, the action of thermo-electric currents, which are produced by unequally heat-

* Poggendorff's *Annalen*, vol. xlvi. p. 585.
† *Comptes Rendus*, vol. xxxviii. p. 905.

ing the two places where platinum and glass are in contact, must be carefully avoided; for this reason the sand-bath was always employed here; the polarizations produced by the actions of galvanic currents are so strong that I could never observe any disturbance caused by thermo-electric currents.

By means of a commutator I investigated the relation which exists between the polarization, the temperature, and the intensity of the current. Two glass bars were heated in the same sand-bath to 250° C., then through the one a current from a single element, through the other a current from two elements of a zinc and platinum battery was passed, until the polarizations each attained a maximum; both bars were afterwards made to produce opposite effects upon a galvanometer with which they were connected by means of a commutator. The polarization produced by two elements was strongest, and similarly it continued to increase according as four, six, or ten elements were employed. The magnitude of the polarization does not depend upon the acting force, but upon the actual intensity; and as this is still small in this case, it explains why, with so great an electromotive force, an increase of polarization always occurred.

Further, the same current was passed successively through a glass bar which was heated to 130°, and through a second heated to a cherry-red heat. By turning the commutator, it was found that the platinum wires which were heated to 130° were most strongly polarized. Accordingly, heated glass before it fuses behaves in every respect like other electrolytes; so that Becquerel's[*] pyro-electric battery differs from common batteries in no other respect, except that in the former the electrolytes have to be prepared by heat, whereas in the latter they are always ready at hand. We cannot estimate what advantage may be thus gained, inasmuch as the same intensity of current corresponds, in both kinds of batteries, to the same destruction of metal[†].

[*] *Comptes Rendus*, vol. xxxviii. p. 905.

[†] Since completing, and for the most part writing down, the above experiments, I have received a memoir from Buff, which has yet to appear in the *Ann. der Chem. und Pharm.* (vol. xc. p. 257), wherein the conducting power of glass is also examined. [See p. 12 of the present volume of this Journal.] Although the experiments have been conducted quite differently, our results agree perfectly, *e. g.* in reference to the commencement of conducting power, to the polarization, and to its dependence upon the intensity of the current and the temperature. Whilst Buff arrived at the last by single measurements, wherein he had to assume the equality of the polarizations which, according to his own statement, had not quite constant values when the currents had different intensities, I discovered the same dependence by means of the commutator; nevertheless, the differences between the polarizations at different temperatures are so great, according to Buff's measurements, that we willingly overlook the small deviations

By the foregoing investigations, therefore, the law that all bodies whose conducting power is increased by heating are electrolytes, is now freed from those exceptions which it appeared to suffer; neither is it necessary, although many may yet feel inclined to do so, to ascribe the apparently incomplete electrolysis to a double conduction, as Foucault has lately assumed, and as Faraday himself has already acknowledged. Even if the hypothesis of such a coexistence of two conductions involves nothing contradictory in itself, it is corroborated by no single experiment. The proofs submitted by Foucault in favour of his views may be considered as disproved by the opposite experiments of Buff*. The experiments communicated by Foucault† at a later period are based upon an erroneous conception of the term branch-current; they can possess no weight whatever, inasmuch as formulæ dependent upon this erroneous conception have been improperly introduced into the observations. At any rate the above experiments show that, even in the weakest conduction by an electrolyte, the latter is electrolytic; and hence that the coexistence of both kinds of conduction can never take place in the manner of branch-conductions, but at most that in the electrolysis every moving atom again conducts.

XXVIII. *On a General Method of Substituting Iodine for Hydrogen in Organic Compounds, and on the Properties of Iodopyromeconic Acid.* By Mr. JAMES F. BROWN, *Assistant to* Professor ANDERSON, *Glasgow*‡.

IN a paper on pyromeconic acid read before this Society, and since published in the Philosophical Magazine for September 1852, I have detailed the preparation and properties of a compound obtained by the substitution of an equivalent of bromine for an equivalent of hydrogen in that acid. Having observed that this substitution was very easily effected, I was induced to attempt the formation of an iodopyromeconic acid, in the hope of adding one to the very few instances in which the direct substitution of iodine for hydrogen has been found possible. For this purpose I digested pyromeconic acid with tincture of iodine, but no success attended the experiment, the acid remaining entirely unchanged. The failure of this attempt led

from the proper values which are caused by introducing those slightly erroneous numbers into calculation. I may also mention, that early in April I had already communicated my results to many of my colleagues in Berlin. (To which I, for my part, gladly bear testimony.—Poggendorff.)

* *Annalen der Chemie und Pharm.* vol. lxxxviii. p. 177.
† *Cosmos*, vol. iv. p. 248.
‡ From the Transactions of the Royal Society of Edinburgh, vol. xxi. part 1; read April 3, 1854.

me to speculate as to its cause, and to contrive a method of producing the required substitution which has proved entirely successful, and has the further advantage of being perfectly general, so that its application will enable chemists to obtain iodine substitution compounds in cases in which they have hitherto failed.

A few preliminary observations on the cause of substitution will render intelligible the nature of the method in question. Selecting the production of bromopyromeconic acid and of trichloracetic acid, as characteristic examples of substitution, we have the following formulæ representing the changes which occur :—

$$C^{10} H^4 O^6 + Br^2 = C^{10} H^3 Br O^6 + H Br$$

Pyromeconic acid. Bromopyromeconic acid.

$$C^4 H^4 O^4 + Cl^6 = C^4 H Cl^3 O^4 + 3 H Cl$$

Acetic acid. Trichloracetic acid.
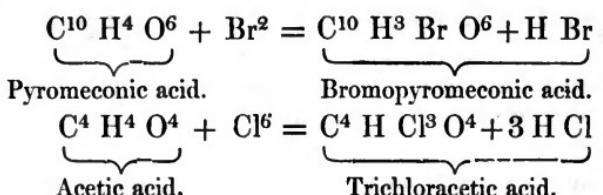
In these, as in every other case of substitution, the chlorine and bromine obviously perform a twofold function, one portion entering into the complex atom in the place of an equivalent quantity of hydrogen, which is eliminated in combination with another quantity either of chlorine or bromine ; and the new product contains the same number of atoms, and is commonly said to belong to the same type. In talking of such substitution, it is not unfrequently said that the atom of chlorine simply displaces or pushes out the atom of hydrogen, and so comes to occupy its place, but a very slight consideration enables us to see that it is really dependent, not so much on the quantity of chlorine which remains in the compound as on that which escapes in combination with hydrogen. The original substance in all cases of substitution, forms a perfect molecular group in which the individual affinities of the different elementary atoms are properly balanced, and the whole remains in a quiescent state. But when another element, such as chlorine, comes in contact with this complex group of atoms, it immediately exerts its affinity for the hydrogen, with which it easily combines, and withdraws the whole or part of it from the compound. In this way a gap is produced in what was before a perfect group, the balance of the affinities of its elements is destroyed, and there must either be a complete readjustment of its molecular arrangements, or some other element must stop the gap and produce another perfect molecule, not differing from the original substance in the arrangement of its parts, but only in the presence of one or more atoms of a

different sort from those which it previously contained. Considered in this point of view, it becomes at once obvious that we fail to produce the substitution of iodine for hydrogen, not from any inability of the former to occupy the place of the latter, but simply because it has not a sufficiently powerful affinity for hydrogen to withdraw it from the compound, so as to leave an empty space into which another portion may enter. It occurred to me that as chlorine and bromine in causing a substitution exercise the two different functions already alluded to, one quantity withdrawing hydrogen, and the other simply slipping into the vacant space, it might be possible to produce an iodine substitution by associating that element with some substance having a sufficiently powerful affinity for hydrogen, to open the door as it were, and leave nothing for it to do but to step into the place prepared for it.

For this purpose I selected the bromide of iodine, as being most likely to fulfill, the required conditions. It was prepared by agitating bromine-water with a considerable excess of iodine, and decanting the reddish-brown solution from the undissolved residue. When this bromide was added to pyromeconic acid, a change rapidly occurred, the solution became colourless, and iodopyromeconic acid was produced. Having succeeded in this way, I then tried the chloride of iodine, and having found it to act equally well, I made use of it in all my subsequent experiments. The chloride of iodine was prepared by passing a rapid current of chlorine through finely-powdered iodine suspended in a small quantity of water and kept in continual agitation, care being taken to stop the process before the iodine was entirely dissolved. A moderate heat is produced during the combination, and the fluid should be kept as cool as possible.

In order, therefore, to obtain iodopyromeconic acid, a freshly-prepared solution of the chloride of iodine is mixed with a cold saturated solution of pyromeconic acid. The solution is instantly decolorized, and the new acid is deposited in the form of fine delicate plates, which are so abundant as to render the fluid almost semisolid. The only precautions necessary are, to avoid the use of a hot solution of the acid, and an excess of the chloride, as the product, under such circumstances, becomes coloured, owing to the occurrence of a further decomposition, to be afterwards referred to. The change which first takes place is represented by the equation—

$$C^{10} H^4 O^6 + I Cl = C^{10} H^3 I O^6 + H Cl.$$

The crystals of iodopyromeconic thus precipitated are in a short time filtered off and washed with cold water; they are then finally dissolved in boiling alcohol, from which they again

deposit themselves, upon cooling, in perfectly colourless plates, having a high degree of lustre. The acid is sparingly soluble in cold water; but at a boiling heat it dissolves more readily, and crystallizes again from the solution in long, slender needles, possessing a slightly acid reaction. Acids and alkalies increase its solubility in water, but it is easily decomposed, if boiled with strong caustic potash. It is also decomposed by concentrated nitric acid, with the separation of free iodine. It gives a yellowish-white precipitate with nitrate of silver, soluble in ammonia, and with perchloride of iron it produces a deep purple colour, but no precipitate. It suffers no loss of weight at 212°, but heated to a higher temperature, it first fuses to a black fluid, and is then suddenly decomposed, with the evolution of a large quantity of iodine.

The combustion of iodopyromeconic acid was attended with some difficulty, for it was found that not only the acid itself, but even its lead salt permitted the iodine to escape in the free state, when burned either with chromate of lead, or with a mixture of oxide of copper and litharge. This would have been of little moment in determining the constitution of a substance such as iodopyromeconic acid, where the mode of its formation sufficiently indicates its composition, and the determination of the carbon and iodine would have been quite sufficient to fix its formula; but having observed the same peculiarity in another substance afterwards to be described, in which the exact determination of the hydrogen was essential to the establishment of its formula, I was compelled to devise some method by which the iodine might be retained, and the following is that which I found most successful.

The substance to be analysed was mixed with chromate of lead and a small quantity of fused litharge reduced to a fine powder; the mixture was then introduced into a long combustion tube, held with its point downwards, and at the same time there were dropped into it small pieces of metallic lead, which remained at the under-side of the tube, and so arranged as to be about 3 inches apart. After the whole of the mixture had been introduced, and the remaining space in the interior of the tube filled up with chromate of lead, the point was turned upwards, and by slight tapping a passage opened throughout the whole length of the tube, while the pieces of lead projecting above the surface melted on the application of heat, and by exposing a metallic surface during the time combustion was going on, served to retain all the iodine.

The results of analysis are as follows:—The hydrogen of No. 1 *was not weighed,* as, from its combustion being effected in the ordinary way without lead, the small end of the chloride of

calcium tube was completely covered with small plates of iodine.

I. 4·893 grains substance, burned with chromate of lead, gave 4·620 grains carbonic acid.

II. 7·298 grains substance, burned with chromate of lead, litharge, and metallic lead, gave 6·759 grains carbonic acid and ·975 grains water.

III. 6·85 grains substance, heated with carbonate of potash, gave 6·707 grains iodide of silver.

	No. 1.	No. 2.	Calculation.		
Carbon	25·750	25·25	25·19	C^{10}	60
Hydrogen	...	1·48	1·26	H^3	3
Oxygen	...	...	20·17	O^6	48
Iodine	52·909	...	53·38	I	127·1
	100·000	100·00	100·00		238·1

These results correspond exactly with the formula $C^{10}H^2IO^5$ + HO.

Iodopyromeconic acid is monobasic, and forms salts, of which I have minutely examined only those of baryta and lead. It does not appear to form an ammonia salt.

Iodopyromeconate of baryta is readily prepared by mixing together alcoholic solutions of acetate of baryta, and of the acid made slightly alkaline with ammonia. After a short time it deposits a fine network of delicate crystals, of little solubility either in cold or hot water or alcohol. It is alkaline to litmus paper, and at 212° it suffers no loss of weight.

The following result was obtained on igniting the salt with sulphuric acid :—4·49 grains substance gave 1·63 grains of sulphate of baryta, which corresponds to the formula BaO, $C^{10}H^2IO^5$ + HO, as shown by the following calculation :—

	Experiment.	Calculation.		
Carbon . . .	...	19·068	C^{10}	60
Hydrogen . .	...	·954	H^3	3
Oxygen . . .	...	15·255	O^6	48
Iodine . . .	...	40·394	I	127·10
Baryta . . .	23·84	24·329	BaO	76·55
	100·00	100·000		314·65

Iodopyromeconate of lead is readily obtained as a fine colourless amorphous precipitate, on mixing alcoholic solutions of the acid and acetate of lead, with the addition of a small quantity of

ammonia. As thus prepared, it is apt to carry down an excess of oxide of lead, which is easily removed by warm acetic acid. It is of sparing solubility in alcohol or water, and becomes highly electrical when rubbed. This salt, contrary to expectation, evolves free iodine, like the acid itself, when burned with chromate of lead; but the phænomenon is probably owing to the decomposition of the salt taking place at a temperature much lower than that sufficient to enable the iodine to combine with lead. For this reason I contented myself with a determination of its oxide of lead, which was effected by igniting the salt, after the addition of a few drops of concentrated sulphuric acid :—6·15 grains substance, dried in the air, gave 2·76 grains sulphate of lead, corresponding to the formula PbO, $C^{10} H^2 IO^5$, as shown by the subjoined calculation :—

	Experiment.	Calculation.		
Carbon	...	17·612	C^{10}	60
Hydrogen . .	...	·587	H^2	2
Oxygen . . .	...	11·743	O^5	40
Iodine	...	37·309	I	127·1
Oxide of lead .	33·03	32·749	PbO	111·56
	100·00	100·000		340·66

I have already mentioned, that when the quantity of chloride of iodine employed is larger than is requisite for the production of iodopyromeconic acid, the fluid acquires a yellow colour, due to the presence of another compound, of very remarkable characters, produced by a further decomposition of pyromeconic acid, and to which I give the name of iodomecone. When potash is gradually added to the fluid, after separation of the iodopyromeconic acid, a blackish precipitate immediately falls, which rapidly dissolves on agitation of the fluid, while a peculiar odour is evolved. After the addition of the potash has been continued for some time, a point is reached at which the precipitate assumes a lighter colour, is no longer dissolved, and is not increased by further addition of the alkali. The precipitate is then filtered from the alkaline fluid, washed with cold water, and purified by repeated crystallization from boiling alcohol. By subsequent experiments, I ascertained that it was easy to convert pyromeconic acid entirely into this compound, by adding a large quantity of chloride of iodine, when the crystals of iodopyromeconic acid at first formed rapidly disappeared, and carbonic acid was evolved. Exactly similar effects are produced by bromide of iodine.

Iodomecone is obtained in large hexagonal plates of a bright

yellow colour and brilliant lustre, and having an odour resembling that of saffron. It is insoluble in water, soluble in alcohol, especially on boiling, and in æther. It is insoluble in hydrochloric acid, and may be boiled with that reagent without suffering decomposition. Strong nitric acid attacks it with great violence, but does not effect a complete oxidation of all the iodine. It is unacted on in the cold by strong sulphuric acid, but if heated, it is decomposed with the liberation of iodine. Caustic potash when long boiled with it, removes a very small quantity of iodine. It does not affect litmus paper, and seems to possess neither acid nor basic properties. It sublimes unaltered at a temperature greatly below that of boiling water.

These characters closely approximate to those of iodoform, and I at first considered it to be that substance. The analysis, however, made with every care, and on specimens prepared at different times, gave results which cannot be made to agree with the formula of that substance.

The analyses are as follows :—

I. 7·141 grains substance, air-dry, gave 0·913 grains carbonic acid and 0·913 grains water.

II. 4·365 grains substance, air-dry, gave 4·365 grains carbonic acid and 0·164 grains water.

III. 8·153 grains substance, air-dry, gave 0·958 grains carbonic acid and 0·346 grains water.

I. 5·697 grains substance, air-dry, gave 9·706 grains iodide of silver.

II. 3·611 grains substance, air-dry, gave 6·140 grains iodide of silver.

	No. 1.	No. 2.	No. 3.
Carbon	3·48	lost.	3·204
Hydrogen . . .	...	0·417	0·471
Oxygen	...	...	4·440
Iodine	...	92·060	91·885
	100·000	100·000	100·000

	Mean.	Calculation.		
Carbon . . .	3·337	3·258	C^6	36
Hydrogen . .	0·444	0·364	H^4	4
Oxygen . . .	4·247	4·344	O^6	48
Iodine . . .	91·972	92·034	I^8	1016·8
	100·000	100·000		1104·8

The formula is therefore $C^6 H^4 I^8 O^6$.

The decomposition by which iodomecone is produced from

pyromeconic acid, now becomes obvious. It is represented by the following equation :—

$$C^{10} H^4 O^6 + 8ICl + 8HO = C^6 H^4 I^8 O^6 + 4CO^2 + 8HCl.$$
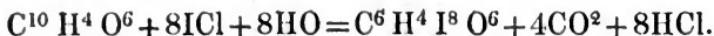
This being the mode of its formation, it seemed probable that meconic and comenic acids, which differ from pyromeconic acid only by the elements of carbonic acid, would yield the same substance when acted on by chloride of iodine. Accordingly, I have found that it is immediately produced with all its characteristic properties from these acids, by the same process. The relations of this substance to meconic and pyromeconic acids, are of a very remarkable character, and cannot at present be distinctly brought out. It obviously belongs to the same class as the very curious product obtained by Cahours, by the action of bromine on citric acid, and which he has called bromoxaform. According to this chemist, when bromine is added to citrate of potash, effervescence takes place from the evolution of carbonic acid, and on the addition of potash, an oily matter is thrown down, which consists of three substances, one bromoform, the other a crystalline solid, bromoxaform, and the third apparently an accidental product, for it is obtained in too small quantities to admit of examination. It can scarcely be doubted that bromoxaform would be the only product, if the action could be properly moderated, and that the bromoform is a secondary product of the former substance, from which indeed it is readily obtained by treatment with caustic potash. If this be the case, the decomposition of citrate of potash would be quite analogous to that of pyromeconic acid, as represented in the equation—

$$C^{12} H^5 K^3 O^{14} + 2HO + Br^{14} = C^6 H Br^5 O^4 + 6CO^2 + 3K Br + 6HBr.$$

$\underbrace{\phantom{C^{12} H^5 K^3 O^{14}}}$
Citrate of potash.
$\underbrace{}$
Bromoxaform.
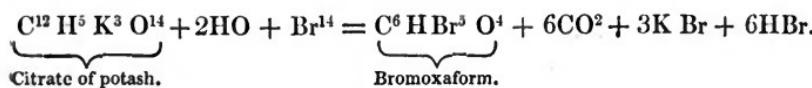
The relation which these curious substances bear to their parent acids is very obscure, and cannot be elucidated without further experiments. In regard to iodomecone, the small quantity in which I was able to obtain it, has prevented my following out its decompositions as I could have wished, but I propose extending this investigation to some of the stronger acids, by which means some light may probably be thrown upon the constitution of these bodies.

It was my desire to have extended my examination of the iodine substitution products obtained by chloride of iodine to some other substances. As yet, however, I have only tried codeine ; but the instability of the compound produced has occasioned such difficulties, that I have hitherto been unable to

arrive at satisfactory results. When chloride of iodine is added to a concentrated solution of hydrochlorate of codeine, a fine yellow crystalline precipitate makes its appearance. It is insoluble in water, but readily soluble in boiling alcohol. If this solution is carefully effected, and too much of the substance be not added, it crystallizes on cooling in stellar groups of a fine red colour, but if a large quantity is dissolved, it is deposited as a perfectly amorphous mass. Unfortunately, the iodine is retained with a very feeble affinity, and I have found that at every crystallization, a small quantity is separated and remains in the fluid, so that results of a satisfactory character could not be obtained on its analysis. It is soluble in hydrochloric acid, and if the solution be made hot, it deposits at first an oily substance, which afterwards concretes to a flocky mass. Both ammonia and potash precipitate it from its solution in hydrochloric acid, the former giving a slightly coloured substance. With chloride of platinum it yields a bright yellow precipitate, one determination of the platinum in which gave 12·20 per cent.; 11·95 corresponds to the formula $C^{36} H^{19} I^2 NO^6 HCl PtCl^2 + HO$, which represents the hydrated salt of a base, which may be called di-iodocodeine, as being derived from codeine by the substitution of two atoms of iodine for two of hydrogen.

These experiments were conducted in the laboratory of Professor Anderson, to whom I am much indebted for assistance during their prosecution.

XXIX. *On the presence of Pyridine among the volatile Bases in the Naphtha from the Bituminous Shale of Dorsetshire, and on the fractional Crystallization of Platinum-salts.* By C. GREVILLE WILLIAMS[*].

IN 1846 we were made acquainted with a new organic base which occurred, together with aniline and leucoline, in coal-tar; subsequently to this, the same substance was found to exist in the *Oleum animale Dippellii*, as procured from bones by distillation: the interesting discovery was also made by Anderson, that this base, to which he gave the name of picoline, was one of a series of nitryle bases having the general formula $(C^n H^{n-5}) N$, of which the first term was pyridine, $C^{10} H^5 N$.

In an examination which I made of the basic part of the Dorsetshire shale naphtha[†], I ascertained the presence of this series of bases from picoline upwards, as far as the one recently discovered, consisting of $C^{16} H^{11} N$. I also showed the presence of a new base still higher in the series, and consisting of $C^{18} H^{13} N$.

The alkaloids were chiefly separated from each other by fractional distillation, but the more volatile of them were present in too small a quantity to render this method applicable; I therefore availed myself of the platinum-salts as a means of purification, and on adding a strong aqueous solution of chloride of platinum to a solution of the first fraction in hydrochloric acid, a precipitate was obtained, which on combustion gave exactly the composition of picoline. But the fraction from which this result was obtained, distilled below and at 210° Fahr., while the boiling-point of picoline, according to Anderson, is 272°; I therefore suspected the presence of a more volatile base, but was prevented by deficiency of material from pursuing the investigation further.

Having since that time obtained another small quantity of liquid from the same source and boiling at the same temperature, I availed myself of the opportunity of studying the separation of bases by fractional crystallization of platinum-salts.

The basic oil, amounting to about three-fourths of a drachm, was supersaturated with hydrochloric acid, an aqueous solution of chloride of platinum added, and the whole diluted until the precipitate at first formed was redissolved; the liquid was then allowed to crystallize spontaneously, and at the end of three days yielded a considerable crop.

On examination these were found to consist of two sorts, *a* and *b*; the former had assumed the shape of sword-blades of a pale yellow colour, and more than half an inch long; *b*, on the other hand, formed small flattened four-sided prisms, very thick in proportion to their length, and of a deep ruby-red. It was, of course, supposed that the salts of two bases had crystallized in the same liquid, but on careful ignition the following numbers were obtained :—

I. $\begin{cases} a.\ 4{\cdot}707 \text{ grs. gave } 1{\cdot}482 \text{ platinum.} \\ b.\ 6{\cdot}022 \text{ grs. gave } 1{\cdot}910 \text{ platinum.} \end{cases}$

Per cent.

a.	*b.*	Theory (*lutidine*).
31·48	31·70	31·51

We therefore see that both salts had the same composition, but the anomaly in the boiling-point, and the difference in the appearance of the crystals, renders the opinion probable that two bases are present, bearing the same relation to each other that picoline does to aniline.

Recent researches having shown the constitution of platinum-salts to be remarkably modified by the action of heat upon their solutions, the evaporations in these experiments were made by exposing them to a surface of sulphuric acid until a crop was obtained, which was then removed and the operation repeated.

It is absolutely necessary before igniting the salts to pulverize the crystals and wash them with a mixture of alcohol and æther; the appearance of the powder becomes much altered by this operation, as it removes a resinous impurity which causes the particles to hang together, while after washing with the mixture indicated, and subsequent drying, the powder becomes as mobile as fine dry sand*. Moreover, it is inadmissible to dry the crystals by heat, a temperature of even 212° having the power to cause a slow evolution of the base, and also appearing to alter the constitution of the salt; it was always dried, therefore, over sulphuric acid until it ceased to lose weight.

The second crop of crystals consisted of large thick prisms.
II. 3·505 grs. gave 1·115 gr. platinum.

Per. cent.	Theory (*lutidine*).
31·81	31·51

The third; long thin square prisms.
III. 3·507 grs. gave 1·130 gr. platinum.

Per. cent.	Theory (*picoline*).
32·22	32·94

The fourth crop; very hard short prisms of a deep red colour.
IV. 4·520 grs. gave 1·478 gr. platinum.

Per. cent.	Theory (*picoline*).
32·69	32·94

The fifth crop; groups of deep orange radiated needles.
V. 5·252 grs. gave 1·823 gr. platinum.

Per. cent.	Theory (*pyridine*).
34·71	34·60

The first and second crops are seen therefore to be alike, also the third and fourth; the first two consisting of lutidine, the next two of picoline, and the fifth of pyridine; the reason of the latter base appearing only in the last crop is in consequence of the smallness of the quantity present. It is interesting to observe the steadiness of the rise of platinum in the fractions until we read the last, when it suddenly springs from 32·7 to 34·7. The assertion in my former memoir that the fractional crystallization of platinum salts affords a far readier mode of purification of homologous volatile bases than distillation, appears, therefore, to be borne out by this result, for it is certain that even with many times the quantity of material, a separation like this could not have been effected by the other method.

It is difficult to explain how a liquid containing a considerable quantity of picoline which boils at 272°, a somewhat less amount of lutidine (boiling-point 310°), and a little pyridine boiling at 240°, should distil below 210° (especially as it was ascertained

* It is believed that Hofmann first indicated the importance of this procedure in his researches on the volatile organic bases.

that no other bases having different per-centages of platinum in their salts were present), unless by supposing the real boiling-points to be much lower than those indicated by their discoverer, and, in fact, I have since ascertained that he does not by any means insist upon their accuracy. If the suggestion thrown out, of the presence of two bases of the composition of lutidine, be correct, we can easily imagine that to be the cause of the discrepancy; but I have obtained in the crystallization of the platinum-salts of æthylamine, crops forming together, and yet having no resemblance in colour or form, some being thin lemon-coloured plates, and others solid deep red prisms, both, however, giving the same results on analysis.

It is somewhat singular that the so-called quinoline, submitted to fractional distillation, yields a base having a constitution apparently closely allied to the pyridine series; in fact one of the fractions converted into platinum salt yielded exactly the quantity of carbon, hydrogen, and platinum in the base between lutidine and parvoline; this result, and some others, has induced me to undertake a rigid examination of quinoline.

It was, unfortunately, impossible to obtain enough platinum-salt of pyridine from the shale products at my disposal for a combustion; but when we consider that in the bases from the same source I have demonstrated the existence of all the other members of the series, and that the fifth fraction gives exactly the theoretical quantity of platinum, we have no reason to doubt the presence of pyridine; the more especially, that although the powerfully-odorous picoline is present, it is unable entirely to conceal the overpowering smell peculiar to the former base.

The difficulties in the way of separating several basic liquids of analogous constitution by fractional distillation is so great, that it would appear quite hopeless to obtain any one product absolutely pure, even where the quantity of material at command is very considerable; but by first fractionating the liquid by distillation and then converting the fractions into platinum-salts and repeatedly crystallizing until the product gives exactly the theoretical quantity of platinum, we may, by distillation with potash, obtain the base in a state fit for examination of its properties.

To show the connexion between the two series, I append the following table from my former memoir.

Unknown	$C^{10} H^5 N$	Pyridine	$C^{10} H^5 N$
Aniline	$C^{12} H^7 N$	Picoline	$C^{12} H^7 N$
Toluidine	$C^{14} H^9 N$	Lutidine	$C^{14} H^9 N$
Xylidine	$C^{16} H^{11} N$	New base	$C^{16} H^{11} N$
Cumidine	$C^{18} H^{13} N$	Parvoline	$C^{18} H^{13} N$

XXX. *On Sensitive Collodion.* *By* THOMAS WOODS, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,   Parsonstown, August 1854.

TO answer some inquiries which have been addressed to me with respect to the photographic process published in the July Number of this Magazine, I beg to make the following addenda.

The mixture of iodide and muriate of iron acts better when it is a few days old; these salts of iron being formed by the double decomposition which occurs between the iodide of potassium and muriate of soda on the one hand, and the sulphate of iron on the other, and the last-named salt being with difficulty dissolved in the alcohol, the action requires some time before the full effect is produced.

The picture developed by the sulphate of iron as an exciting agent is a positive one, and if rapidly taken, not deep enough to print from. The usual pyrogallic acid solution (3 grains to the ounce of water, with half a drachm of glacial acetic acid) brings out the picture much more deeply marked. With the pyrogallic acid developing solution I have taken a likeness in about $1\frac{1}{2}$ second which could be printed from very fairly.

I have found from late experiments that the intensity of the picture may be considerably increased by using collodion without the salt recommended in the paper published last July, but substituting for it one drop of *chloroform* to the mixture when about to be poured on the glass plate; that is, mixing together 1 part of the solution of iodide of iron, 3 parts of collodion, and 1 drop of chloroform to each drachm of this compound fluid when about to be poured on the plate. The first effect of the chloroform on the collodion is to precipitate the gun-cotton which comes in contact with it, but it soon redissolves by giving a shake or two to the vessel.

I am, &c.,

THOMAS WOODS.

XXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 155.]

June 15, 1854.—The Earl of Rosse, President, in the Chair.

THE following papers were read :—

"Examination of the Cerebro-spinal Fluid." By William Turner, Esq., Scholar of St. Bartholomew's Hospital.

In the Bulletin de l'Académie de Médecine for December 1852, a paper is published by M. Bussy, containing an analysis by M. Des-

champs of a fluid, which flowed from the ear of a man who had sustained a fracture of the base of the cranium. From a comparison between the composition of this fluid and that given by M. Lassaigne as the composition of the cerebro-spinal fluid, M. Bussy arrived at the conclusion that they were identical in their origin. In addition, however, to the albumen and ordinary saline constituents, M. Deschamps found that the fluid obtained from the fractured cranium contained a certain constituent which possessed the peculiar property of reducing the blue protoxide of copper to the state of the yellow suboxide.

As this power of reducing the oxide of copper is possessed also by grape-sugar, M. Bussy arrived at the conclusion that this fluid contained a small portion of grape-sugar, and as additional evidence in support of this conclusion he quotes the experiments of M. Bernard, who, by irritating the base of the encephalon and the origin of the vagus nerve, produced an excess of sugar in the secretions. He supposes that in the present instance the fracture through the base of the cranium may have produced some irritation at the origin of the pneumogastric, and thus have excited the formation of sugar. Such a supposition would have received additional confirmation if at the same time an analysis could have been made of the blood, urine, or other secretions, so as to determine if sugar was present in those fluids—no such analysis however is given. The property of reducing the oxide of copper was also found by M. Bussy to reside in the cerebro-spinal fluid of the Horse and Dog. In none of these experiments was he able to induce fermentation. As this reducing power is not peculiar to grape-sugar, but is possessed by other organic substances, such as lactine and lactucine, this test alone should not be relied on as affording any positive indications of its presence; recourse should therefore be had to other confirmatory experiments.

With a view to determine this point, Mr. Paget, in the early part of March last, gave me for examination three separate portions of the cerebro-spinal fluid, obtained by puncturing a spina bifida in a child, several days intervening between the removal of each portion.

Those removed on the first two occasions were perfectly clear and pellucid, giving an alkaline reaction to test-paper, their spec. grav. being 1·006, no spontaneous coagulation taking place after standing for some time; that removed on the third occasion had a slightly yellow tinge, and a distinct coagulum formed in it on standing. The presence of fibrine in this instance was owing doubtless to some slight inflammation having been set up, caused by the successive puncturings. The three specimens corresponded in the following characters:—

1st. No precipitate on applying heat, merely an opalescence being produced; on the addition of a few drops of nitric acid a white flaky precipitate subsided. Nitric acid alone, without heat, also caused a precipitate.

The non-precipitation of the albumen, until the addition of the acid, was owing to the alkalinity of the fluid.

· 2nd, Boiled with liquor potassæ a very faint pinkish tint was produced; a few white flakes also fell down.

3rd. Heated in a water-bath with the blue oxide of copper, in a few minutes the yellowish red powdery suboxide precipitated.

This reaction took place both in the original albuminous liquid and after the coagulation of the albumen by heat and nitric acid.

4th. A piece of flannel, saturated with the chloride of tin, was well moistened with the fluid, and then heated over a red-hot coal; no brown colour of the flannel was produced, such as occurs when grape-sugar is present. (Maumenè's test.)

5th. A portion mixed in a test-tube with some German yeast was placed for several hours in a warm cupboard, but there was no development of gas.

From these experiments it appears that of the various tests employed, only one gave any indication of the presence of grape-sugar, that test also being the one which is most liable to deception. The lowness of the specific gravity, in which respect this fluid and that analysed by M. Deschamps closely corresponded, would, *à priori*, almost lead to the assumption that no grape-sugar was present.

The presence of the reducing agent could not in this case depend upon any irritation of the origin of the vagus, for the irritation, if any, produced by a spina bifida is at the end of the cerebro-spinal axis furthest removed from the origin of that nerve. That the material however which effects this reduction is of a very changeable nature, was shown by allowing a portion of the fluid to stand for several days until putrefaction had commenced. The fluid was then filtered so as to separate the insoluble albuminous flakes, and the clear liquid heated in a water-bath with the blue oxide of copper; when, instead of the suboxide being produced, the black anhydrous oxide was formed, just as is the case when the blue oxide is heated merely with water, thus satisfactorily showing that the reducing substance had been destroyed.

The recent investigations of Virchow* and Busk† have shown that substances of a non-nitrogenous nature exist both in the brain and spinal cord, but they hold somewhat different opinions respecting their exact characters; for whilst the former considers them to be cellulose, the latter regards them both in their "structural, chemical and optical properties" to resemble starch. In conformity with these views, it was interesting to determine if any indications of the presence of either of these substances could be found in the cerebro-spinal fluid; accordingly a portion of the fluid was evaporated nearly to dryness and then divided into two portions; to one was added an alcoholic solution of iodine and concentrated sulphuric acid, when a violet tint was produced, which after a few minutes disappeared; but it was also found that this same appearance was produced when the acid and iodine solution were mixed together alone, the violet colour being evidently owing to the volatilization of a part of the iodine and the evolution of its characteristic violet tint; to the other a solution of iodide of potassium and then nitric acid was

* Quarterly Journal of Microscopical Science, January 1854.　　† Ibid.

added, when a brown colour was produced, owing to the liberation of the iodine. In neither portion could it be said that any evidence of the presence of starch or cellulose was detected.

A comparative trial was also made between the effects produced upon the blue oxide of copper by the cerebro-spinal fluid, solutions of grape-sugar, cane-sugar, starch, cellulose, and mannite, an unfermentizable sugar. These various substances were heated in a water-bath for the same length of time, when it was found that whilst the grape-sugar effected a reduction immediately, and the cerebro-spinal fluid only after the lapse of several minutes, neither the starch, cellulose, cane-sugar nor mannite effected any reduction at all.

The power of reducing the blue oxide of copper is not confined to non-nitrogenous substances, for I found that if a solution of leucine* be heated along with it in the usual manner, the reduction is effected in about the same length of time, and in the same way as by the cerebro-spinal fluid. This single experiment is not of itself sufficient evidence that the reducing power in both cases depends upon the presence of the same substance. Such an assertion could only of course be proved by obtaining from the cerebro-spinal fluid leucine in the crystallized form. A proper quantity of the fluid was not, however, left to investigate this point.

From the above experiments I think it may be safely asserted that the power possessed by the cerebro-spinal fluid of reducing the oxide of copper, is not owing to the presence either of grape-sugar or any of the allied substances : whether it may depend upon the presence of leucine or other modifications of albumen of a somewhat similar nature, or whether it may be due to the presence of a substance belonging to another series, is a point that has yet to be determined.

Note by Mr. Paget.—The patient from whom the fluid analysed by Mr. Turner was obtained, was a girl born of healthy parents. An infant cousin had lately died from the same congenital defect as she presented. The upper part of the body was well formed, but the pelvis and lower limbs were small and nearly powerless. The sac containing the fluid was seated over the last lumbar vertebra, projecting (as the examination after death showed) through an opening between its unclosed arches. It enlarged quickly after birth, but did not evidently affect the child's health, unless it were connected with a very frequent spasmodic action of the muscles closing the glottis, which, almost from the time of birth, had produced the peculiar " crowing inspiration," or laryngismus stridulus. The fluid was first withdrawn when the child was three months old. Neither on this, nor on any subsequent occasion, did its removal produce any manifest effect, although the flaccidity of the emptied sac indicated that the pressure upon the spinal cord was greatly diminished. After every evacuation the sac very quickly filled again, notwithstanding pressure exercised upon it.

* Leucine $C^{12} NH^{13} O^4$, a weak base, belonging to the same series as glycocine and alanine, is generally obtained by the decomposition of albuminous substances. It has been obtained by Scherer from the spleen, and, according to Gregory, has been detected as a natural product in the liver of the Calf.

The examination after death showed that the fluid was collected in the expanded tissue of the pia mater, or subarachnoid spaces, about the cauda equina. The pia mater presented appearances of inflammation long past, as well as of that which had probably been the cause of death. The canal in the axis of the spinal cord was distinct in its whole length. Commencing, below a large fourth ventricle, with a diameter of about one-fourth of a line, it gradually widened, till, at the lumbar part of the cord, it had a diameter of a line and a half. Its termination at the end of the cord could not be traced in the confusion of parts caused by the distension and inflammation of the membranes.

" On the Oxidation of Ammonia in the Human Body." By H. Bence Jones, M.D., F.R.S.

In the last edition of Professor Lehmann's Animal Chemistry, vol. ii. p. 363, a very decided opinion is expressed against the conclusion to which I arrived in consequence of some experiments published in the Philosophical Transactions for 1851.

I considered it proved that ammonia was partly at least converted into nitrous acid in its passage through the body. In opposition to this Professor Lehmann states,—

1st. That the method which I employed must of necessity give a reaction resembling that given by nitrous acid; his words are, " Es wäre nun leicht einzusehen dass schweflige Säure, durch welche bekanntlich Iodwasserstoff zersetzt wird, in die Vorlage übergeht und so jene vermeintliche salpetersaure Reaction bedingt."

2ndly. That when nitric acid was added to urine and it was distilled with phosphoric acid instead of sulphuric acid, no trace of blue colour with starch and iodide of potassium could be obtained. " Das nach Anwendung von Phosphorsäure erhaltene Destillat giebt aber auch jene vermeintliche salpetersaure Reaction nicht, ja selbst dann nicht, wenn dem Harn vorher absichtlich einige Tropfen Salpetersäure zugesetzt worden waren."

It appeared to me undesirable merely to reply to Professor Lehmann, that I had expressly stated that the indigo and protosulphate of iron tests were used, and gave as decided proof of the presence of nitrous acid in the urine as Price's test gave; and that sulphurous acid could not have produced the same effect as nitrous acid in these tests. It seemed more desirable to repeat the experiments which had been made in Professor Lehmann's laboratory on the action of sulphurous acid, and on the effect of using phosphoric instead of sulphuric acid in the distillation of the urine.

I was fortunate enough to obtain the assistance of Mr. Malone to carry on the experiments continuously from day to day, and through the kindness of Dr. Hofmann this was done in the College of Chemistry.

1st. On the action of sulphurous acid on starch and iodide of potassium and very dilute hydrochloric acid.

In England it is by no means well known that sulphurous acid decomposes hydriodic acid. On the contrary, theoretically it should not liberate iodine, and experimentally not only does it not liberate

iodine, but it hinders the liberation of iodine and stops the formation of the blue colour when Price's test is used and nitrous acid is present; and if sulphurous acid be added after the blue colour is formed it makes it disappear.

Pure sulphurous acid was prepared, some nitre was fused, and a dilute solution was made, and it was tested by Price's test (starch, iodide of potassium and very dilute hydrochloric acid), then the dilute nitre solution immediately gave the deep blue iodide of starch; but when much or little sulphurous acid was added previously to the nitre solution, no blue colour at all was produced; and when, instead of the nitre solution, much or little sulphurous acid alone was added, contrary to the statement of Lehmann, no decomposition of the hydriodic acid could be obtained.

If instead of pure iodide of potassium it was mixed with iodate of potassa, an immediate blue colour was of course observed. I can only suppose that in this way Professor Lehmann obtained the reaction which he has attributed wrongly to the action of sulphurous acid on hydriodic acid, unless indeed no sulphurous acid at all was present and the acidity of the distillate was unneutralized. Dr. Lehmann is however right as well as wrong, in saying that Price's test for nitric acid fails when sulphurous acid is present. The test fails, not, as he says, because sulphurous acid has the same action as nitrous acid in liberating iodine, but because it has exactly the opposite property of hindering the iodide from being set free even when nitrous acid in small quantity is present.

It is possible that in distilling the urine with sulphuric acid, the distillation, if carried too far, may give rise to sulphurous acid, and that thus Price's test may fail to detect nitrous acid in the urine. Moreover, portions of the distillate may be projected against the sides of the hot retort, by which the sulphuric acid acting on the organic matter may be decomposed, and minute quantities of sulphurous acid may be liberated. This sulphurous acid, instead of decomposing hydriodic acid, causes the reformation of hydriodic acid when nitrous acid liberates iodine in Price's test.

2ndly. Lehmann states that experiments were made by distilling urine to which a few drops of nitric acid were added with phosphoric acid, and that then the distillate gave no reaction with Price's test. The following experiments were made with every precaution.

Anhydrous phosphoric acid was prepared, and it was found to be free from nitrous acid. Some healthy urine was taken and some pure nitrate of potassa, in the proportion of two grains of salt to an ounce of fluid, and distilled with phosphoric acid (ten ounces of urine, twenty grains of nitre, and one ounce of anhydrous phosphoric acid). On concentrating, the neutralized dilute nitrous acid was detected by all the tests, namely, the indigo test, the protosulphate of iron and Price's test.

In a second experiment, five ounces of urine with five grains of nitre and half an ounce of anhydrous phosphoric acid, gave nitrous acid by all the tests. The distillation was continued until the contents of the retort were viscid.

In a third experiment, three ounces of urine with a grain and a half of nitre were distilled with three drachms of glacial phosphoric acid; the distillate neutralized and evaporated gave no trace of nitrous acid; the same urine with the same quantity of nitre and three drachms of sulphuric acid, when distilled, gave a distillate, which when neutralized and evaporated gave decided evidence of nitrous acid.

In my former paper I showed that by distilling with sulphuric acid when only one-tenth of a grain of nitre was added to each ounce of urine, nitrous acid could be detected.

From these experiments it appears that distillation with sulphuric acid is to be preferred to distillation with phosphoric acid; but even with this last acid, when a grain of nitre is added to an ounce of urine, the nitrous acid can be detected.

I then endeavoured, by using phosphoric instead of sulphuric acid in distilling urine passed after a salt of ammonia had been taken into the stomach, to detect nitrous acid in the urine.

Two drachms of muriate of ammonia were taken in seven ounces of distilled water. The urine was collected for six hours afterwards. Twelve ounces of this urine were distilled with one ounce of phosphoric acid (anhydrous). The distillate, when concentrated, did not give any evidence of nitrous acid by Price's test.

The same experiment was repeated with no better result.

In another experiment, sulphuric acid, six drachms to twelve ounces of urine, was used instead of phosphoric acid. The distillate as soon as it was obtained gave the slightest precipitate with chloride of barium insoluble in nitric acid, showing that a trace of sulphuric acid was carried over into the receiver. The distillate was made alkaline with pure carbonate of soda, evaporated, and nitrous acid was immediately detected by the indigo and iron test, as well as by Price's test. A portion of the distillate left exposed to the air, on the following day had lost the power of liberating iodine. This arose from the nitrous acid passing into nitric acid.

Pure nitre gives no colour with starch, iodide of potassium and dilute hydrochloric acid, but when fused it produces the liberation of iodine immediately. If the solution of fused nitre is exposed to the air it loses this property, but regains it when the solution is evaporated to dryness and refused and again dissolved.

In another experiment six ounces of urine passed before the muriate of ammonia was taken were distilled with half an ounce of sulphuric acid, the distillate was highly acid, and gave a slight precipitate with chloride of barium; it was made slightly alkaline, evaporated to a small residue, and then gave no evidence of nitrous acid. Then two drachms of muriate of ammonia were taken in seven ounces of distilled water, eight ounces of urine passed four hours afterwards were distilled with half an ounce of sulphuric acid. The distillate was fractional; the first portion gave no colour with starch test; it contained a minute trace of sulphurous acid. The second portion was highly acid; it was made slightly alkaline, evaporated nearly to dryness, and then gave most positive evidence of nitrous

acid by Price's test, and also by decolorizing a deep solution of indigo.

Thus before the salt of ammonia was taken no nitrous acid could be detected in the urine, whilst after the ammonia nitrous acid was proved to be present, not only by Price's test, but by the indigo test also.

In conclusion, it results from these experiments,—1st, That in Price's test sulphurous acid produces exactly the opposite effect to nitrous acid, and even hinders nitrous acid from liberating iodine from hydriodic acid.

2ndly. That phosphoric acid, when mixed with urine containing nitre and distilled very low, does liberate nitrous acid; though when used instead of sulphuric acid, it does not enable the nitrous acid to be detected so readily as when the latter acid is employed.

Hence the experiments performed in Professor Lehmann's laboratory by Herr Jaffé*, do not invalidate Price's test for nitrous acid in the way Professor Lehmann supposes; and by again repeating some of my former experiments, I still arrive at the conclusion that when ammonia is taken into the body nitric acid may be detected in the urine, but that the quantity which can be made to appear is so small that the most delicate method is required for its detection. This however is no proof that a much larger quantity may not be lost in the process for obtaining it from the urine.

"On the Disintegration of Urinary Calculi by the Lateral Disruptive Force of the Electrical Discharge." By George Robinson, M.D., Licentiate of the Royal College of Physicians of London, and Lecturer on Medicine in the Newcastle-upon-Tyne College of Practical Science.

The great and diversified powers of electricity have long suggested the possibility of its being employed as a means of effecting the destruction of calculi in the human bladder, and thus obviating the necessity for the painful and dangerous operation of lithotomy. But the attempts hitherto made in this direction have contemplated the solution of the stone through electrolytic action rather than its disintegration by the mechanical force of the electrical discharge. A moment's reflection will however suffice to convince us that the force which shatters a steeple or cleaves an oak, is also capable of reducing to fragments the largest urinary concretion. Nor can I imagine any other than the following sources of objection to the practicability of employing this force for the purpose of breaking down vesical calculi *in situ*, namely, 1. the danger to the living structures from the necessity of using a powerful discharge; 2. the difficulty of conveying the force to the required spot, or in other words, causing the discharge to pass through the calculus. The first objection is in a great measure met by the fact of our being enabled to regulate with the utmost precision the degree of intensity of the discharge, and it would be almost entirely removed were it possible to apply the disruptive force of electricity without any portion of the body being included within the circuit traversed by the electrical current. The

* Erdmann's Journal, vol. lix. p. 238, 1853.

second objection rests upon the mechanical difficulty of bringing the calculus within the direct route of the electrical discharge, but would scarcely apply were it demonstrated that the disruptive effects of electricity can be obtained without any such direct transmission of the current.

My own attention was some years since directed to the subject by reading an account of the following experiment first performed by Mr. Crosse. " Two platinum wires one-thirtieth of an inch in diameter were secured to a slip of window glass half an inch wide and four inches long, so that they rested upon the flat surface of the glass, leaving an interval between their points of one-twentieth of an inch. The wires were connected, one with the negative con- ductor of a powerful machine, the other with a ball to receive sparks from the prime conductor. On placing the glass in a flat dish filled with water and turning the machine, the glass between the points soon became fractured, and after 100 revolutions the fracture enlarged and two small cracks appeared. After 200 revolutions an excavation was formed, but on the side *opposite* to that on which the wires were tied. After 250 revolutions the glass was completely perforated. Many variations of this experiment were made, in all of which the same kind of mechanical effect was obtained. Even quartz was excavated*."

It being thus shown that a lateral disruptive action takes place within a certain distance of the seat of discharge, the idea at once suggested itself to me, that by using two parallel wires separated at their extremities like those in Mr. Crosse's experiment, and similarly connected with an electrical machine or Leyden jar, bringing their ends in contact with the surface of a calculus, and then allowing a series of moderate discharges to take place between the extremities of the wires, a disintegrating effect would be produced upon urinary calculi of the same nature as that witnessed in glass and quartz. And short of the actual disintegration of a calculus in the bladder of a living person, the following experiments will, I trust, be deemed conclusive on this point.

Two copper wires, one-twentieth of an inch in diameter, were connected, one with the external, the other with the internal surface of a Leyden jar, having about 400 square inches of internal metallic coating. These copper wires were soldered to platinum wires half an inch long and one-thirtieth of an inch in diameter. Each wire was drawn through a fine gutta percha tube, and the tubes, having first been placed perfectly parallel, were warmed and gently pressed together so as to assume somewhat of the appearance of a flexible bougie; the platinum wires projecting beyond the gutta percha to the extent of one-eighth of an inch, and their free extremities being slightly everted and separated from each other by an interval of one- tenth of an inch. In experimenting, the united gutta percha tubes were grasped and the projecting platinum points pressed against the surface of the calculus: the jar was then discharged by another

* Described by Mr. Walker in Lardner's Cabinet Cyclopædia, vol. ii. pages 218–220.

person, and a series of such discharges thus passed between the free extremities of the parallel platinum wires while resting upon the surface of the stone.

With this simple arrangement, fragments a quarter of an inch long were broken off flints immersed in water, and the same force was applied to urinary calculi with the following results :—

Exp. 1. June 7th.—A piece of a large lithic acid calculus was placed in a bladder, nearly filled with water, into which the gutta percha bougie containing the wires was then introduced and the neck of the bladder tied round the instrument. The bladder with its contents being placed on a wet board, the projecting platinum wires were then kept in contact with the surface of the calculus and the jar discharged. On opening the bladder and examining the stone, it was found to be broken into numerous fragments by the single discharge.

Exp. 2.—A small phosphatic calculus, very smooth and hard, was experimented upon in a similar manner. The first five discharges produced no perceptible effect, but the sixth split it into at least twenty fragments, and many of these, on being slightly pressed between the finger and thumb, readily broke down.

Exp. 3.—A very large oxalate of lime or mulberry calculus with projecting tubercles was similarly tested, and the first discharge produced a small cavity in the surface to which the wires were applied, separating a considerable quantity of fine sand; but subsequent discharges did not act so efficiently on this very large stone.

Exp. 4.—On the following day, June 8th, the experiment was repeated in the presence of Messrs. Potter, Rayne and Furness, surgeons in Newcastle, and a small calculus, removed a few months since by the gentleman last mentioned from a young boy, was, after a few trials, split through the centre, one-half being reduced to fragments, and the other exhibiting in its interior a dark-coloured nucleus of lithic acid.

These experiments appear to demonstrate the practicability of applying the lateral disruptive force of the electrical discharge to the disintegration of calculi in the bladder. There can be no difficulty in bringing the end of a gutta percha catheter, conveying two copper wires, in contact with the surface of a stone in the bladder, and a very simple mechanical contrivance will enable the extremities of the platinum wires to be protruded when the end of the catheter touches the calculus. By employing two wires, one connected with the positive, the other with the negative, portion of the jar or machine, not only is the intensity of the discharge increased, but the body is also prevented from forming any part of the circuit, and the risk of injury thereby materially diminished. The bladder used in the above-mentioned experiments was not at all injured, and on retaining a portion of it between the platinum wires so that the discharge passed through it, no perforation or other destructive effect took place. The gutta percha tubes, having the projecting platinum wires, were placed in the mouth without being in contact with the lips, and a discharge sent through the wires, but there was no per-

ceptible shock. When, however, the bladder containing the stone rested upon the hand, during the act of disintegration a smart impulse was felt.

On the whole, I am of opinion that the electrical force applied in the manner indicated, will be found quite as efficient for the disintegration of calculi in the bladder as the more formidable analogous operation of lithotrity, occasionally practised. And, as regards simplicity and security, the electrical apparatus certainly appears preferable to the instruments used for crushing the stone by ordinary mechanical force.

" The Attraction of Ellipsoids considered generally." By Mathew Collins, Esq., B.A.

The author commences by stating, that the attraction of an ellipsoid on a point on its surface or within it, in a direction perpendicular to one of its principal planes, is proportional to the distance of the attracted point from that plane.

This general proposition, which is an extension to *ellipsoids* of those already given for *spheroids* in Airy's Tract " On the Figure of the Earth," Prop. 8 and 10, and in MacLaurin's 4th Lemma, " De causa physica Fluxus et Refluxus Maris," he demonstrates—

1. In the case when the attracted point is on the surface of the ellipsoid.

The demonstration of this is much like those given by the above-named authors for the less general case of spheroids, and its final step is effected by Cor. 1 to Prop. 87 of the first book of the Principia.

2. When the attracted point is within the ellipsoid.

The demonstration in this case is effected by showing that an ellipsoidal shell, bounded by two similar and similarly placed ellipsoidal surfaces, exerts no attraction on a point situated anywhere within it or upon its interior surface.

The foregoing proposition shows that the attraction of an ellipsoid on any point on its surface, or within it, can be got at once from the attraction of the same ellipsoid on a point placed at the extremity of an axis, and the author proceeds to show how the latter attraction can be found and reduced to elliptic functions. He then gives this proposition :

Let a, b, c be the semiaxes of a homogeneous fluid ellipsoid, and A, B, C the forces acting on points at the extremities of a, b, c, caused partly by the ellipsoid's own attractions on its parts, and partly by centrifugal forces of revolution about an axis ($2c$), or by the action of an extraneous force directed towards its centre, and varying as the distance from the centre, then the ellipsoid will preserve its form if $Aa = Bb = Cc$.

The last proposition stated in the paper is thus given : let R and r be the radii of two homogeneous concentric spheres; A and a the attractions of each on a point on the surface of the other, then $\frac{A}{R^2} = \frac{a}{r^2}$, whatever be the law of attraction as a function of the distance.

The demonstration given of the first of these two theorems is very concise, and of the second is direct and elementary.

"Researches on the Impregnation of the Ovum in the Amphibia; and on the Early Stages of Development of the Embryo." (Third Series.) From the MS. papers of the late George Newport, F.R.S., F.L.S. &c. Selected and arranged by George Viner Ellis, Esq., Professor of Anatomy in University College, London.

In this paper the author has given the result of further inquiries into the manner by which the frog's egg is impregnated, and has supplied in addition some very interesting facts respecting the development of the embryo during the earlier stages of its growth.

In consequence of the difficulties that arose in the course of the inquiry, and of the doubts that might be suggested by others from the difficulty of manipulating with the egg of the Amphibia unless certain precautions are taken, the author first describes the apparatus used and the mode of proceeding he has employed; and his results show that he has successfully surmounted the obstacles to microscopic investigation caused by the opacity, the great size, and the tendency to movement inherent in the egg.

The fact of the impregnation of the ovum through the entrance of the spermatozoon into the yelk by its own movement was communicated to the Royal Society in a preceding paper*, and the original experiments there referred to as serving to establish the fact, are now detailed. In addition, the circumstances affecting the passage of the sperm-body through the thick investing envelopes are considered, and thence it is concluded, that "when there is any deficiency in the usual power, arising from an unhealthy condition of the fertilising body, or an increase in the resistance of the yelk membranes, the spermatozoon is unable to pass through the membranes into the yelk and the egg remains unfertilized."

The two small rounded bodies that appear on the surface of the yelk in the interval or *chamber* between it and the investing membrane, have been traced from their origin, through their changes, till their disappearance after the equatorial division of the yelk. The investigations as to the true import of these bodies have not been further carried out, in consequence of the untimely death of the author; but his observations have induced him to put forth the following statement regarding them, viz. "that they are usually, and perhaps invariably, at that part of the yelk at which the head of the embryo is afterwards found."

By following the changes in the segmenting yelk, evidence has been obtained of the derivation of different parts of the future being from definite segments of the yelk. Thus it has been found, that the half of the yelk on one side of the second or crucial cleft begins its subdivisions sooner than the opposite, and that the trunk and tail of the embryo are derived from this first subdividing part, whilst the head is produced from the other half.

Having ascertained so much respecting the foundation of different parts of the embryo, the author next determined that the axis or

* Philosophical Transactions for 1853, p. 271.

spine will primarily lie in a line with the first cleft of the yelk, though it may afterwards deviate somewhat from that line during the growth of the embryo.

Lastly, it has been sought to discover what influence the artificial application of the spermatozoon to only one side of the egg would have upon the direction of the primary cleft of the yelk. The result of this inquiry seems, very curiously, to be, that the first cleft of the yelk will lie, under the circumstances stated, in a line with the point of the egg that has been touched with the impregnating fluid.

"Contributions to the Anatomy of the Brachiopoda." By Thomas H. Huxley, F.R.S.

In the course of the dissection of certain Brachiopoda with which I have recently been engaged, I have met with so many peculiarities which are unnoticed in the extant and received accounts of their anatomy, that although the pressure of other duties prevents me from attempting to work out the subject with any degree of completeness for the present, I yet gladly avail myself of the opportunity of communicating a few of the more important results at which I have arrived, in the hope that they may find a place in the Proceedings of the Royal Society.

My investigations were principally made upon *Rhynchonella psittacea*, for specimens of which I am indebted to Prof. Edward Forbes, while Dr. Gray obligingly enabled me to compare them with *Waldheimia flavescens* and with *Lingula*.

1. *The Alimentary Canal of Terebratulidæ.*—Professor Owen, in both his earlier and his later memoirs on the anatomy of the Terebratulidæ, describes at length the manner in which the intestine, as he states, terminates on the right side between the lobes of the mantle.

On the other hand, Mr. Hancock has declared himself unable to observe at this point any such anal aperture, and concludes from his own observations that the latter is situated on the ventral surface of the animal in the middle line, just behind the insertion of the great adductor muscle. M. Gratiolet, in a late communication to the Académie des Sciences, takes the same view. To get rid of the obvious difficulty, that this spot is covered by the shell, and therefore that if the anus existed here, there would be no road of escape for the fæces, Mr. Hancock and Mr. Woodward appear to be inclined to suppose that some cloacal aperture must exist in the neighbourhood of the pedicle.

The existence of any such aperture, however, has recently been denied with great justice by Professor Owen.

The result of my own repeated examinations of *Rhynchonella psittacea* and of *Waldheimia flavescens* is—1. that the intestine does not terminate on the right side of the mantle as Professor Owen describes it, but in the middle line, as Mr. Hancock describes it in *Waldheimia*, while in *Rhynchonella* it inclines, after curving upwards, to the *left* side; and 2. that there is no anus at all, the intestine terminating in a rounded cæcal extremity, which is straight and conical in *Waldheimia*, curved to the left side and enlarged in *Rhynchonella*.

I confess that this result, so exceptional in its character, caused

me no small surprise, and I have taken very great pains to satisfy myself of the accuracy of my conclusion; but notwithstanding the strong prejudice to the contrary, to which the known relations of the anal aperture in *Lingula* gave rise, repeated observation has invariably confirmed it.

Professor Owen's statement is, that in *Rhynchonella (Terebratula) psittacea* " the intestine inclines to the right side and makes a slight bend forwards before perforating the circumscribing membrane in order to terminate between the mantle lobes on that side."—*On the Anatomy of the Brachiopoda*, p. 152.

I find, on the contrary (figs. 1 and 2), that the intestine passes

Fig. 1.
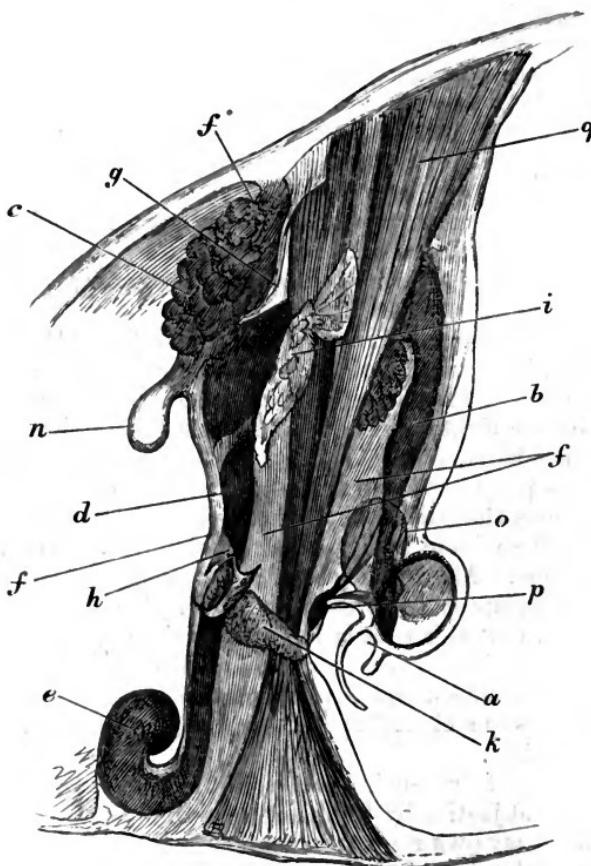
Fig. 1. *Rhynchonella psittacea*, viewed in profile; the lobes of the mantle and the pedicle being omitted.

Fig. 2. The same viewed from behind, the pedicle having been cut away. The left half of the body and the liver are omitted.

a. mouth; *b.* œsophagus; *c.* stomach and liver; *d.* intestine; *e.* imperforate rectum; *f.* mesentery; *g.* gastro-parietal bands; *h.* ilio-parietal bands; *i.* superior 'heart'; *k.* inferior 'heart'; *l.* genital bands; *m.* openings of pallial sinuses; *n.* pyriform vesicle; *o.* sac at the base of the arm; *p.* ganglion; *q.* adductors.

at first straight downwards in the middle line, as in *Waldheimia*, but instead of terminating in a rounded tapering extremity as in that genus, it bends upwards and then curves round to the *left* side, forming a sort of free cæcum in the visceral cavity. My reasons for believing that it is a free cæcum are these :—in the first place, no anal aperture can be detected in the mantle cavity, either on the right or left sides, although the small size of the animal allows of its being readily examined uninjured, with considerable magnifying powers.

Secondly. If the shell be removed without injuring the animal and the visceral cavity be opened from behind by cutting through its walls close to the bulb of the pedicle, it is easy not only to see that the disposition of the extremity of the intestine is such as I have described it to be, but by gentle manipulation with a needle to convince oneself that it is perfectly unattached. And in connexion with this evidence I may remark, that the tissues of the Brachiopods in general are anything but delicate ; it would be quite impossible for instance to break away the end of the intestine of *Lingula* from its attachments without considerable violence.

Fig. 2.
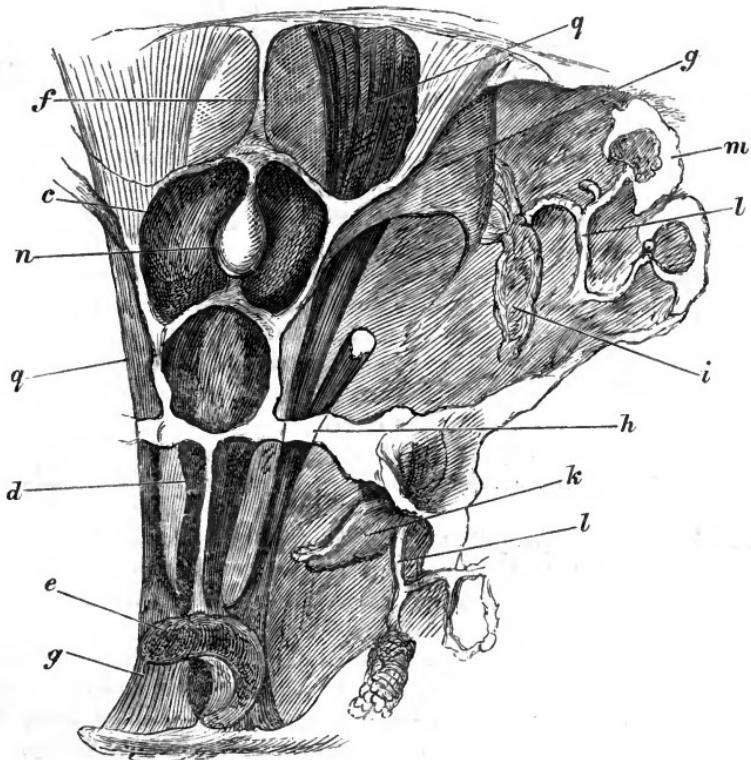
Thirdly. If the extremity of the intestine, either in *Rhynchonella* or in *Waldheimia*, be cut off and transferred to a glass plate, it may

readily be examined microscopically with high powers, and it is then easily observable that its fibrous investment is a completely shut sac. In *Rhynchonella* the enlarged cæcum is often full of diatomaceous shells, but it is impossible to force them out at its end, while if any aperture existed they would of course be readily so extruded.

However anomalous, physiologically, then, this cæcal termination of the intestine in a molluscous genus may be, I see no way of escaping from the conclusion that in the *Terebratulidæ* (at any rate in these two species) it really obtains. There are other peculiarities about the arrangement of the alimentary canal, however, of which I can find either no account at all or a very imperfect notice.

The intestinal canal (figs. 1 and 2 *b, d, e*) has an inner, epithelial, and an outer fibrous coat; the latter expands in the middle line into a sort of mesentery, which extends from the anterior face of the intestine between the adductors, to the anterior wall of the visceral chamber, and from the upper face of the intestine to the roof of the visceral chamber; while posteriorly it extends beyond the intestine as a more or less extensive free edge. I will call this the *mesentery (f)*.

From each side of the intestinal canal, again, the fibrous coat gives off two 'bands,' an upper (*g*), which stretches from the parietes of the stomach to the upper part of the walls of the visceral chamber, forming a sort of little sheath for the base of the posterior division of the adductor muscle, which I will call the *gastro-parietal band*; and a lower, which passes from the middle of the intestine to the parietes, supporting the so-called '*auricle*.' I will call this the *ilio-parietal* band (*h*).

The ilio-parietal and gastro-parietal bands are united by certain other ridges upon the fibrous coat of the intestine, from whose point of union in the middle line of the stomach posteriorly, a pyriform vesicle (*n*) depends.

The mesentery divides the liver into two lateral lobes, while the gastro-parietal bands give rise to the appearance that these are again divided into two lobules, one above the other. I am inclined to think that these bands are what have been described as 'hepatic arteries,' at least there is nothing else that could possibly be confounded with an arterial ramification upon the liver.

This description applies more especially to *Rhynchonella* and *Waldheimia*, but the arrangement in *Lingula* is not essentially different.

2. *The Circulatory System of Terebratulidæ.*—Considerable differences of opinion have prevailed among comparative anatomists as to the nature and arrangement of the vascular system in the Brachiopoda. A pair of organs, one on each side of the body, have been recognized as Hearts since the time of Cuvier, who declared these hearts in *Lingula* to be aortic, receiving the blood from the mantle and pouring it into the body, the principal arterial trunks being distributed into that glandular mass which Cuvier called ovary, but which is now known to be the genital gland of either sex.

Professor Owen in his first memoir follows Cuvier's interpretation, stating that in *Orbicula* the pallial veins terminate in the hearts,

from which arterial branches proceed to the liver and ovary. Professor Owen further adds for the Brachiopoda in general,—

" Each heart, for example, in the Brachiopoda is as simple as in Ascidia, consisting of a single elongated cavity, and not composed of a distinct auricle and ventricle as in the ordinary Bivalves," and he compares the hearts of Brachiopoda to the auricles of Arca, &c. (Trans. Zoological Society, vol. i. p. 159).

In 1843, however, M. Vogt's elaborate memoir on *Lingula* appeared, in which the true complex structure of the ' heart' in this genus was first explained and the plaited ' auricle' discriminated from the ' ventricle;' and in 1845, Professor Owen, having apparently been thus led to re-examine the circulatory organs of Brachiopoda, published his ' Lettre sur l'appareil de la Circulation chez les Mollusques de la Classe des Brachiopodes,' in which he felicitates M. Milne-Edwards on the important confirmation of the views which the latter entertains with respect to the lacunar nature of the circulation in the Mollusca, afforded by the Brachiopoda, and describes each heart of the Terebratulidæ as consisting of a ventricle and a plaited auricle, the pallial veins not terminating in the latter but in the general visceral cavity.   As the Professor does not recal the view which he had already taken of the circulation in *Orbicula*, I presume that he considers two opposite types of the circulatory organs to obtain in the Brachiopoda, the direction of the current being from the mantle through the heart towards the body in *Orbicula*, and from the mantle through the body towards the heart in *Terebratula*.

The possibilities of nature are so various that I would not venture, without having carefully dissected *Orbicula*,—no opportunity of doing which has yet presented itself,—to call this view in question, but I think it seems somewhat improbable.   Indeed the structural relations which I have observed and which are described below, do not appear to me to square with any of the received doctrines of Brachiopod circulation, but I offer them simply as facts, not being prepared at present to present any safe theory on the subject.

In *Waldheimia flavescens* there are two ' hearts,' situated as Professor Owen describes them, but so far as I have been able to observe, the ventricle cannot be described as an ' oval' cavity, inasmuch as it is an elongated cavity bent sharply upon itself.   Hastily examined of course this may appear oval.   I have been similarly unable to discover ' the delicate membrane of the venous sinuses,' which is said by Professor Owen to " communicate with and close the basal apertures of the auricles," or to perceive that the auricular cavity can be " correctly described as a closed one, consisting at the half next the ventricle, of a beautifully plicated muscular coat in addition to the membranous one, but at the other half next the venous sinus of venous membrane only ; the latter might be termed the auricular sinus, the former the auricle proper."

I presume that ' this delicate membrane of the venous sinuses ' is what I have called the ilio-parietal band, in which the base of the auricle is as it were set, like a landing-net in its hoop, but this does *not* close the base of the auricle, the latter opening widely into the visceral chamber.

I have equally failed in detecting any arteries continued from the apices of the ventricles; and I have the less hesitation in supposing I have not overlooked them, as Mr. Albany Hancock, whose works are sufficient evidence of the value of his testimony, permits me to say that he long since arrived at the conclusion that no such arteries exist.

What has given rise to the notion of the existence of these arteries appears to me to be this. A narrow band resembling those I have already described, is attached in *Waldheimia* along the base of the 'ventricle' and the contiguous outer parietes of the auricle: inferiorly it passes outwards to the sinuses, and running along their inner wall, forms a sort of ridge or axis* from which the genitalia, whether ovaria or testes, are developed, stretching through their whole length and following the ramifications of the sinuses. It is the base of these ridges seen through the walls of the sinuses, where they extend beyond the genitalia, which have been described as arteries.

The upper end of the band passes into the sinuses of the upper lobe of the mantle, and comes into the same relation with the genitalia which they enclose.

The walls of the auricle in *Waldheimia* are curiously plaited, but I have been unable, in either auricle or ventricle, to detect any such arrangement of muscular fibres as that which has been described. The epithelial investment of the auricle, on the other hand, is well developed, and in the ventricle the corresponding inner coat is raised up into rounded villous eminences.

The ventricle lies in the thickness of the parietes, while the auricle floats in the visceral cavity, supported only by the ilio-parietal band. The former is at first directed downwards, but then bends sharply round and passes upwards to terminate by a truncated extremity close to the suboesophageal ganglion and bases of the arms.

Mr. Hancock informs me, that in his dissections he repeatedly found an aperture by which the apex of the 'ventricle' communicated with the pallial cavity; and that, taking this fact in combination with the absence of any arteries leading from this part, he had been tempted to doubt the cardiac nature of these organs altogether, and to regard them rather as connected with the efferent genital system, had not the difficulty of determining whether these apertures were artificial or natural prevented his coming to any definite conclusion at all.

Before becoming acquainted with Mr. Hancock's investigations, I had repeatedly observed these apertures in *Rhynchonella*, but preoccupied with the received views on the subject, I at once interpreted them as artificial. A knowledge of Mr. Hancock's views, however, led me to reconsider the question, and I have now so repeatedly observed these apertures both in *Waldheimia* and in *Rhynchonella*, that I am strongly inclined to think they may after all be natural.

If these organs be hearts, in fact, *Rhynchonella* is the most remarkable of living Mollusks, for it possesses *four* of them. Two of these occupy the same position as in *Waldheimia*, close to the origins of the calcareous crus (k), while the other two are placed above these, and

* This arrangement is, I find, particularly described by M. Gratiolet.

above the mouth, one on each side of the liver (*i*). It is these latter which Professor Owen describes, while he has apparently overlooked the other two, at least he says (speaking as I presume of *Rhyncho-nella*) (*l. c.* p. 148) that the venous sinuses "enter the two hearts or dilated sinuses which are situated exterior to the liver, and in *T. Chilensis* and *T. Sowerbii* just within the origins of the internal calcareous loop."

The fact is, that while the ilio-parietal bands support two 'hearts' as usual, the gastro-parietal bands are in relation with two others. The base of the 'auricle' of the latter opens into the re-entering angle formed by the gastro-parietal band with the parietes, while its apex is directed backwards to join the ventricle, which passes downwards and backwards along the posterior edge of the posterior division of the adductor muscle.

The auricles in *Rhynchonella* are far smaller, both actually and proportionally, than in *Waldheimia*. They exhibit only a few longitudinal folds, and not only present the same deficiency of muscular fibres as those of *Waldheimia*, but are so tied by the bands which support them that it is difficult to conceive how muscular fibres, even if they existed, could act. The 'ventricles' in like manner lie obliquely in the parietes of the body, and simply present villous eminences on their inner surface, which has a yellowish colour.

All these 'hearts' exhibit the same curious relation with the genitalia in *Rhynchonella* as in *Waldheimia*; that is to say, a 'genital band' (*l*) proceeds from the base of the 'ventricle' and becomes the axis of the curiously reticulated genital organ. But in *Rhynchonella* the genital bands of the upper genitalia come from their own 'hearts.'

The arrangement of the genitalia in *Rhynchonella* is very remarkable. The sinuses have the same arrangement in each lobe of the mantle. The single trunk formed by the union of the principal branches in each lobe opens into the inner and anterior angle of a large semilunar sinus which surrounds the bases of the adductors, and opens into the visceral cavity. The floor of this great sinus is marked out into meshes by the reticulated genital band, and from the centre of each mesh a flat partition passes, uniting the two walls of the sinus, and breaking it up into irregular partial channels.

There are the same anastomosing bands uniting the gastro-parietal and ilio-parietal bands on the stomach in *Rhynchonella* as in *Waldheimia*, and a pyriform vesicle of the same nature, but I did not observe in *Rhynchonella* those accessory vesicles upon the origins of genital bands, which I observed once or twice in *Waldheimia*.

I could find no trace of arteries terminating the elongated, ovoid and nearly straight 'ventricles' of *Rhynchonella*; their ends appeared truncated, and as I have already said, repeatedly presented a distinct external aperture.

Such appear to me to be the facts respecting the structure of the so-called hearts in the *Terebratulidæ*; what I believe to be an important part of their peripheral circulatory system, has not hitherto, so far as I am aware, received any notice.

In *Waldheimia* the membranous walls of the body, the parieto-intestinal bands and the mantle, present a very peculiar structure; they consist of an outer and an inner epithelial layer, of two corresponding fibrous layers, and between them of a reticulated tissue, which makes up the principal thickness of the layer, and in which the nerves and great sinuses are imbedded.

The trabeculæ of this reticulated tissue contain granules and cell-like bodies, and I imagined them at first to represent a fibro-cellular network, the interspaces of which I conceived were very probably sinuses. Sheaths of this tissue were particularly conspicuous along the nerves. On examining the arms, however, I found that the oblique markings, which have given rise to the supposition that they are surrounded by muscular bands, proceeded from trabeculæ of a similar structure, which took a curved course from a canal which lies at the base of the cirri (not the great canal of the arms, of course) round the outer convexity of the arm, and terminated by breaking up into a network. These trabeculæ, however, were not solid but hollow, and the interspaces between them were solid. The network into which they broke up was formed by distinct canals, and then, after uniting with two or three straight narrow canals which ran along the outer convexity of the arm close to its junction with the interbrachial fold, appeared to become connected with a similar system of reticulated canals which occupied the thickness of that fold.

It was the examination of the interbrachial fold, in fact, which first convinced me that these reticulated trabeculæ were canals; for it is perfectly clear that vessels or channels of some kind must supply the proportionally enormous mass of the united arms with their nutritive material, and it is so easy to make thin sections of this part, that I can say quite definitely that no other system of canals than these exists in this locality.

The *facts*, then, with regard to the real or supposed circulatory organs of the *Terebratulidæ*, are simply these :—

1. There are two or four organs (hearts), composed each of a free funnel-shaped portion with plaited walls, opening widely into the visceral cavity at one end, and at the other connected by a constricted neck, with narrower, oval or bent, flattened cavities, engaged in the substance of the parietes. The existence of muscular fibres in either of these is very doubtful. It is certain that no arteries are derived from the apex of the so-called ventricle, but whether this naturally opens externally or not is a point yet to be decided.

2. There is a system of ramified peripheral vessels.

3. There are one or more pyriform vesicles.

4. There are the large 'sinuses' of the mantle, and the 'visceral cavity' into which they open.

To determine in what way these parts are connected and what functions should be ascribed to each, it appears to me that much further research is required.

Nervous System of Terebratulidæ.—Professor Owen describes and figures the central part of this system as a ring surrounding the oral

aperture, its inferior portion being constituted by a mere commissural band.

M. Gratiolet, however, states with justice that the inferior side of this collar is the thicker, and I find both in *Rhynchonella* and in *Waldheimia* that it constitutes, in fact, a distinct oblong ganglion, of a brownish colour by reflected light. From its extremities commissural branches pass round the mouth, while other cords are distributed to the arms, to the superior and inferior pallial lobes, and to the so-called hearts. The nerves are marked by fine and distinct longitudinal striations, and can be traced to the margins of the pallial lobes, where they become lost among the muscular fibres of the free edges of the mantle.

Structure of the Arms.—I have not been able to convince myself of the existence of that spiral arrangement of the muscular fibres of the arms which has been described in *Rhynchonella* and *Waldheimia*. I have found the wall of the hollow cylinder of the arm to be constituted (1) externally, by an epithelium, within which lie (2) the reticulated canals, which have been already described; (3) by a delicate layer of longitudinal or more oblique and transverse fibres, which are probably muscular, and (4) internally by a granular epithelial layer.

In *Rhynchonella* the bases of the arms are terminated by two considerable sacs, which project upwards into the visceral cavity. Have these the function of distending and so straightening the spirally coiled, very flexible arms of this species?

Affinities of the Brachiopoda.—All that I have seen of the structure of these animals leads me to appreciate more and more highly the value of Mr. Hancock's suggestion, that the affinities of the Brachiopoda are with the Polyzoa. As in the Polyzoa, the flexure of the intestine is neural, and they take a very natural position among the neural mollusks between the Polyzoa on the one hand, and the Lamellibranchs and Pteropoda on the other.

The arms of the Brachiopoda may be compared with those of the Lophophore Polyzoa, and if it turns out that the so-called hearts are not such organs, one difference will be removed.

In conclusion, I may repeat what I have elsewhere adverted to, that though the difference between the cell of a Polyzoon and the shell of a Terebratula appears wide enough, yet the resemblance between the latter with its muscles and the Avicularium of a Polyzoon, is exceedingly close and striking.

"On the frequent occurrence of Indigo in Human Urine." By Arthur Hill Hassall, M.D.

From the present communication it appears that the occurrence of indigo in human urine is more common than the author was led to anticipate from his first inquiries.

The author furnishes additional proofs of the blue colouring matter in question being really indigo, by converting it into isatine and aniline.

Contrasting its chemical and physiological relations with hæmatine and urine pigment, he shows that indigo is closely allied in its

nature and origin to those substances, and he considers that when indigo is met with in urine in considerable amount, it forms a vehicle for the elimination of any excess of carbon contained in the system. This view is borne out by the important fact, that the greater number of cases in which indigo has been observed to be developed in the urine in large amount have been cases of extensive tubercular disease of the lungs, and in which the decarbonizing functions of those organs are greatly impaired.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

May 1, 1854.—A paper was read by Professor De Morgan on the Convergency of Maclaurin's Series, being an Appendix to a paper on some Points in the theory of differential equations.   See the abstract of the former paper, Phil. Mag. vol. vii. p. 450.

Mr. Kingsley made an oral communication on the Chemical Nature of Photographic Processes.

May 15.—A paper was read by Mr. Warburton on Self-repeating Series.

In computing Bernoulli numbers by the formula of Laplace[*], the author of this paper was led to notice, that in the fraction whose development is a series of the form $1^{2n+1} - 2^{2n+1}t + 3^{2n+1} \cdot t^2 - \&c.$, the numerator of that fraction is a recurrent function of t.   This led him to investigate the question, what are the conditions which the denominator of the generating fraction, and the terms of the series generated, must satisfy, in order that the numerator of such a fraction may be a recurrent function of t.   The paper contains the result of that investigation.

The author calls those series "*self-repeating*," which, when extended without limit in opposite directions, admit of separation into two similar arms, each arm beginning with a finite term of the same magnitude.   Between this pair of finite terms, either no zero-term, or one or more zero-terms, may intervene.   One arm repeats, and contains arranged in reverse order, the terms of the other arm, either all, or none, of the terms having their signs changed.   The different positive integer powers of the natural numbers, of the odd numbers, and of the figurate numbers of the several orders, present familiar examples of self-repeating recurring series.

The author demonstrates the following three theorems respecting self-repeating recurring series :—

I. If the series arising from the development of a proper fraction is the right arm of a self-repeating recurring series, and if the denominator of such a fraction is a recurrent function of t, then the numerator also is a recurrent function of t.

II. Other things remaining the same, if the numerator of the fraction is a recurrent function of t, then the denominator also is a recurrent function of t.

* See Memoirs of the Academy of Sciences, 1777.

III. If the numerator and the denominator of a proper fraction are each a recurrent function of *t*, then the series, arising from the development of the fraction according to the positive integer powers of *t*, will be the right arm of a self-repeating recurring series.

By way of example, the author applies his first theorem to the summation of the infinite series $1^9 - 2^9 + 3^9 - \&c.$, and compares his process with the corresponding processes of Laplace and of Sir John Herschel. The sum in question is given by Sir John Herschel (see Jameson's Journal, January 1820) in terms of the differences of the powers of 0, extending from $\Delta^1 0^9$ to $\Delta^9 0^9$. In the author's process, the requisite differences extend from $\Delta^1 0^9$ only to $\Delta^5 0^9$, and the numerical coefficients of these are of diminished magnitude, and of very easy determination.

The author makes other applications of his theorems; but on these we forbear to enter.

A paper was read by Professor Challis on the Determination of the Longitude of the Cambridge Observatory by Galvanic Signals.

The experiment of which this paper contains the details, was made at the suggestion of the Astronomer Royal, and conducted according to a scheme arranged by him for giving and receiving the signals. A galvanic connexion having been established between the Greenwich Observatory and the Cambridge Telegraph Office, by means of the London central station of the Electric Telegraph Company, signals were sent on the nights of May 17 and 18, 1853, between 11^h and 12^h mean time. The signals were made by causing two needles, one at Greenwich, the other at Cambridge, to start by completing the galvanic circuit at either place of observation. The times of starting were noted at both places, and reduced to the sidereal times of the respective observatories, to serve by comparison for determinations of the difference of their longitudes. On each night the signals were made alternately for a quarter of an hour at one station, in batches containing an arbitrary number of signals not exceeding nine, and then for a quarter of an hour at the other station in a similar manner. On the first night the total number of signals was 151, and on the second night 139. The two observers, Mr. Dunkin of the Greenwich Observatory, and Mr. Todd of the Cambridge Observatory, changed places in the interval between the two nights' observations; Mr. Todd observing at Greenwich, and Mr. Dunkin at Cambridge, on the second night. Also it was arranged that the two observers should observe identical stars on the two nights, as well as the stars ordinarily used for clock errors, and that the same apparent right ascensions of the stars should be employed for reducing the signal-times at both observatories. The Cambridge Observatory time was conveyed with the greatest care to the Telegraph Office at the Cambridge Railway Station by the transfer of three chronometers. By a first calculation, the longitude of the Cambridge Observatory was found to be $23^s \cdot 03$ east of Greenwich.

Professor Challis subsequently made another calculation, taking into account the effect on the times of meridian transits of stars produced by the forms of the transit-pivots, according to a method

which he has described in the Memoirs of the Royal Astronomical Society (vol. xix. p. 103). The errors arising from the deviation of the pivots from the cylindrical form being eliminated, the longitude is found to be $22^s \cdot 70$ east of Greenwich, which is less by $0^s \cdot 84$ than the value hitherto adopted.

May 29.—A paper was read by Professor Fisher, entitled "Additional Observations on the Development of the Vertebral System."

XXXII. *Intelligence and Miscellaneous Articles.*

ON THE ARTIFICIAL PRODUCTION OF TAURINE. BY A. STRECKER.

THE properties of taurine convinced me that we should one day succeed in preparing it artificially. M. Redtenbacher has already attempted to produce taurine by means of aldehyde and bisulphite of ammonia, but he only obtained an isomerous body with different properties. With the same view I undertook the following experiments.

Sulphate of methyle, $C^2 H^3 O . SO^3$, furnishes with ammonia, sulphomethylane and wood-spirit, according to the formula

$$2(C^2 H^3O . SO^3) + NH^3 = C^2 H^5 NS^2 O^6 + C^2 H^4 O^2.$$
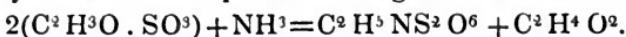
With sulphate of æthyle we might therefore hope to obtain taurine if the decomposition were analogous,—
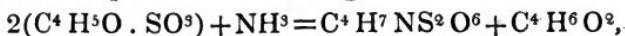
$$2(C^4 H^5O . SO^3) + NH^3 = C^4 H^7 NS^2 O^6 + C^4 H^6 O^2,$$
Sulphate of æthyle.   Taurine.

but I found that sulphate of æthyle behaves in a different manner to sulphate of methyle; a conjugate acid is formed, described by me some years ago under the name of sulphæthamic acid.

Substituting methylamine for ammonia, sulphate of methyle might, from analogy, furnish taurine; thus—
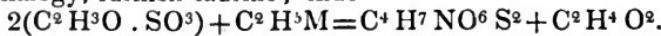
$$2(C^2 H^3O . SO^3) + C^2 H^5M = C^4 H^7 NO^6 S^2 + C^2 H^4 O^2.$$
Sulphate of   Methylamine.   Taurine.
methyle.

I did not try this mode of formation, as I was convinced by experiment that taurine does not give a trace of methylamine on its decomposition with potash.

Nitrite of æthyle, $C^4 H^5O . NO^3$, placed by M. Gerhardt amongst the nitryle compounds $\left. \begin{matrix} C^4 H^3 \\ NO^4 \end{matrix} \right\}$, would produce taurine (or its isomer) by the action of bisulphite of ammonia, if it behaved like nitro-naphthaline. According to M. Piria, with nitro-naphthaline and bisulphite of ammonia we have—

$$C^{20} H^7 NO^4 + 6SO^2 + 2HO = C^{20} H^9 NS^2 O^6 + 4SO^3.$$
Nitro-naphthaline.   Naphthionic acid.
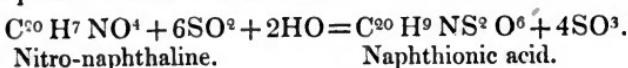
By analogy we should get with nitrous æther,—

$$C^4 H^5 NO^4 + 6SO^2 + 2HO = C^4 H^7 NS^2 O^6 + 4SO^3.$$
Nitrous æther.   Taurine.
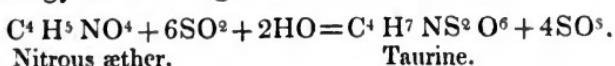
Experiment proves that nitrite of æthyle does not act like nitronaphthaline with bisulphite of ammonia; nitrogen is evolved, and a formation of sulphuric and æthylosulphuric acids takes place.

Isæthionic acid, prepared according to M. Regnault, by means of anhydrous sulphuric acid and olefiant gas, when in combination with ammonia only differs from taurine in composition by two equivs. of water,—
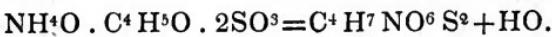
$$NH^4O . C^4 H^5O . 2SO^3 = C^4 H^7 NO^6 S^2 + HO.$$
Isæthionate of ammonia.   Taurine.

This salt fuses at 248° F. without disengaging ammonia, and it might be hoped that at a still higher temperature it would lose water. I first ascertained that taurine might be heated to 464° F. without decomposition or fusion. Isæthionate of ammonia heated to 392° F. began to lose weight; I heated it to 446° F., and kept it at this temperature until it had lost 11 per cent. of its weight. The mass was dissolved in water; on the addition of alcohol it is precipitated in crystals; this precipitate, dissolved in water, furnished by spontaneous evaporation large crystals exactly identical with the crystals of taurine prepared from bile. Like taurine, they bear exposure to a temperature of 464° F. without fusing or acquiring colour; they evolve no ammonia with a solution of potash; they do not precipitate the salts of baryta when boiled with nitric acid or nitromuriatic acid. When fused with potash and nitrate of potash, they evolve ammonia, and the mass contains sulphuric acid. All these properties being the same as those of taurine, and its mode of formation proving that its composition is similar, this product is identical with the taurine of the bile.—*Comptes Rendus*, July 3, 1854, p. 61.

ON THE ELECTRO-CHEMICAL DECOMPOSITION OF WATER.
BY F. LEBLANC.

With the hope of obtaining a gas much charged with ozone by the electrolysis of water, I arranged a voltameter so as to keep it in action in a freezing mixture. In this way, water containing at least one-tenth of its volume of concentrated sulphuric acid was decomposed at a low temperature by means of four ordinary Bunsen's elements. Although the electrodes were formed of simple platinum wires, the volume of oxygen collected was much less than half the volume of the hydrogen disengaged in the same time at the negative pole.

The oxygen was strongly ozonized, but the proportion of ozone absorbable by spongy silver did not warrant the attribution of the diminution in the volume of gas collected at the positive pole, to any difference in volume between ozone and ordinary oxygen.

I observed that the liquid in the voltameter had acquired new properties—energetic oxidizing action; it whitened sulphuret of lead, and superoxidized hydrated oxides like oxygenated water.

I continued these researches, and ascertained the existence of some peculiar phænomena of oxidation, by employing spongy platinum at the positive pole, and placing oxidizable substances in the cold vol-

tameter ; such as acetate of potash, which was rapidly converted into the formiate, &c.—*Comptes Rendus*, March 6, 1854, p. 444.

ON SOME COMBINATIONS OF HYDRARGYROMETHYLE AND HY-DRARGYRÆTHYLE. BY A. STRECKER.

Frankland found a short time since, that when iodide of methyle and iodide of amyle in contact with mercury are exposed to the influence of the solar rays, crystals are formed of which the composition is expressed by the formulæ $C^2 H^3 Hg^2 I$ and $C^{10} H^{11} Hg^2 I$, and to which he gave the names of iodide of hydrargyromethyle and iodide of hydrargyramyle. He did not succeed in producing an analogous compound with iodide of æthyle, although the existence of this combination might be presumed.

Before I was acquainted with Frankland's experiments, I had already obtained the iodide of hydrargyræthyle, which may easily be produced from a mixture of iodide of æthyle and mercury under the influence of diffused light. After some time crystals are formed, of which the quantity increases until the whole of the liquid sets into a mass. The crystals dissolve in boiling æther and alcohol, from which they separate in thin, colourless, shining laminæ. They sublime at 212° F., but only fuse at a higher temperature. They do not dissolve in water, but are soluble in ammonia and in solution of potash, from which they crystallize again without decomposition. On analysis I found their composition to be $C^4 H^5 Hg^2 I$. With nitrate of silver they furnish iodide of silver and nitrate of hydrargyræthyle, $C^4 H^5 Hg^2 O . NO^5$, which crystallizes on evaporation in colourless prisms. Chloride of sodium precipitates chloride of hydrargyræthyle, $C^4 H^5 Hg^2 Cl$, from the watery solution of the nitrate ; it cannot be distinguished from the iodide by its characters. These æthyle compounds are decomposed by sun-light, and this is the reason why Mr. Frankland did not obtain them. I have also prepared the nitrate of hydrargyromethyle ; its composition is $C^2 H^3 Hg^2 O . NO^5 + HO$. —*Comptes Rendus*, July 3, 1854, p. 57.

ON THE ARTIFICIAL PRODUCTION OF CINNAMON OIL. BY A. STRECKER.

I pointed out some years since, that styrone, which is obtained by treating styracine with a concentrated or alcoholic solution of potash, is the *alcohol* of cinnamic acid, as it exhibits the same relation with regard to this acid as alcohol to acetic acid. By experiments made at my suggestion, M. Wolff has proved that styrone, under the influence of energetic oxidizing agents, becomes converted into cinnamic acid.

I have found that styrone, in the same conditions in which alcohol is converted into aldehyde, furnishes the aldehyde of cinnamic acid, or cinnamon oil.

For this purpose it is sufficient to moisten platinum black with

liquid styrone, and leave the mixture exposed to the air. In a few days the greater part of the styrone is converted into cinnamic aldehyde, which is separated from the unaltered styrone by the excellent process of M. Bertagnini. With a concentrated solution of bisulphite of potash, crystals are obtained, which are washed with æther to free them from the styrone. The crystals are purified by the addition of sulphuric acid diluted with pure cinnamic aldehyde. The crystals dissolve in nitric acid, and in a few moments crystals are formed of the nitrate of the hydruret of cinnamyle.

The conversion of styrone into hydruret of cinnamyle by the oxygen of the air is explained by the equation—

$$C^{18} H^{10} O^2 + 2O = C^{18} H^8 O^2 + 2HO.$$
$$\text{Styrone.} \qquad\qquad \text{Hydruret of cinnamyle.}$$
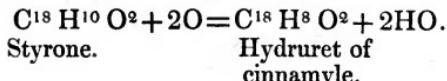
—*Ibid.* p. 61.

Chiswick.—July 1. Heavy rain: overcast. 2. Very fine: slight rain. 3. Slight rain: fine. 4. Densely clouded. 5. Showery. 6. Very fine: heavy showers. 7. Overcast: heavy showers. 8. Rain: very fine: clear. 9. Very fine: cloudy. 10. Showery: heavy rain at night. 11. Cloudy. 12. Drizzly: densely overcast. 13. Very fine. 14. Overcast: clear. 15. Densely clouded: very fine. 16. Very fine. 17. Rain: very fine. 18—21. Very fine: air very dry. 22. Quite cloudless. 23. Clear and calm. 24. Sultry: lightning at night. 25. Very hot: lightning at night. 26. Slight haze: cloudy: rain. 27. Easterly haze: rain. 28. Clear and fine. 29. Slight fog: cloudy. 30. Very fine: thunder-storm 2 to 5 P.M. 31. Uniformly overcast: heavy thunder clouds.

Mean temperature of the month .... 61°·59
Mean temperature of July 1853 .... 61 ·94
Mean temperature of July for the last twenty-eight years ... 63 ·17
Average amount of rain in July .... 2·42 inches.

Boston.—July 1. Cloudy: rain A.M. 2. Cloudy. 3. Cloudy: rain P.M. 4. Fine: rain P.M. 5. Fine. 6, 7. Fine. 8. Cloudy: rain A.M. 9. Cloudy. 10. Cloudy: rain A.M. and P.M. 11. Cloudy. 12. Rain A.M. and P.M. 13. Cloudy. 14, 15. Cloudy: rain A.M. and P.M. 16. Fine. 17. Cloudy: rain A.M. and P.M. 18. Cloudy: rain A.M. 19, 20. Cloudy. 21. Fine: thermometer 84° 5 P.M. 22—24. Fine. 25—29. Cloudy. 30. Fine. 31. Cloudy: rain A.M. and P.M.

Sandwick Manse, Orkney.—July 1. Cloudy A.M. and P.M. 2. Cloudy A.M.: showers P.M. 3. Cloudy A.M.: rain P.M. 4. Rain A.M.: fog P.M. 5. Cloudy A.M.: hazy P.M. 6. Bright A.M.: rain P.M. 7. Clear A.M.: clear, fine P.M. 8. Bright, fine A.M.: cloudy P.M. 9. Drizzle A.M.: cloudy P.M. 10. Bright A.M.: cloudy P.M. 11. Cloudy A.M. and P.M. 12. Drizzle A.M. and P.M. 13. Rain A.M.: damp P.M. 14. Damp A.M. and P.M. 15. Rain A.M.: clear, fine P.M. 16. Cloudy A.M.: cloudy, fine P.M. 17. Damp A.M.: clear, fine P.M. 18. Clear, fine A.M.: fine, fog P.M. 19. Hazy A.M.: cloudy P.M. 20, 21. Clear, fine A.M. and P.M. 22. Showers A.M.: clear P.M. 23, 24. Cloudy A.M. and P.M. 25. Drizzle A.M.: cloudy P.M. 26. Showers A.M.: clear, fine P.M. 27. Clear A.M.: cloudy, fine P.M. 28. Clear, fine A.M. and P.M. 29. Clear, fine A.M.: cloudy, fine P.M. 30. Cloudy A.M.: fog P.M. 31. Fog A.M. and P.M.

Mean temperature of July for twenty-seven previous years ... 55°·08
Mean temperature of this month .... 55 ·25
Mean temperature of July 1853 .... 58 ·15
Average quantity of rain in July for fourteen previous years . 2·40 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1854. July.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston.	Orkney, Sandwick.		Chiswick.		Boston.	Orkney, Sandwick.		Chiswick.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
	Max.	Min.	8½ a.m.	9½ a.m.	8½ p.m.	Max.	Min.	8½ a.m.	9½ a.m.	8½ p.m.	1 p.m.					
1.	29·918	29·878	29·940	29·87	29·87	64	49	59	52	51½	w.	nw.	calm	·02	·35	
2.	29·949	29·876	29·946	29·81	29·72	69	55	64	55	51½	sw.	s.	se.	·02		
☽ 3.	29·805	29·620	29·930	29·63	29·48	72	56	66	55½	52	sw.	ssw.	e.	·02		·06
4.	29·620	29·556	29·908	29·33	29·41	69	46	62	55½	53½	sw.	sw.	nne.	·01	·08	·41
5.	29·702	29·662	29·918	29·45	29·63	67	47	67	57	55	sw.	sw.	e.	·06	·01	·05
6.	29·725	29·695	29·924	29·74	29·76	67	41	67	59	54	s.	sw.	e.	·07		
7.	29·743	29·722	29·930	29·94	29·91	66	51	65	57	54	s.	sw.	n.	·15		1·01
8.	29·750	29·721	29·930	29·99	29·93	72	45	60	57½	53½	e.	nw.	nw.	·05	·09	
9.	29·773	29·723	29·928	29·99	30·04	72	49	64	53	50½	w.	nw.	n.	·02		
○ 10.	29·909	29·861	29·944	30·02	30·01	67	51	56	52	50½	n.	n.	nw.	·40		·03
11.	29·905	29·864	29·946	29·97	29·98	66	50	58	54	50	nw.	nw.	nw.	·01	·17	·01
12.	29·851	29·790	29·938	29·89	29·82	60	46	53	51½	53	sw.	n.	wnw.	·17	·23	
13.	29·854	29·774	29·942	29·72	29·66	68	49	61	54	54½	sw.	wnw.	wsw.		·07	·15
14.	29·924	29·749	29·930	29·61	29·63	65	56	64	54½	54½	nw.	ssw.	w.	·02		·01
15.	30·096	29·769	29·933	29·73	29·87	72	47	68	54½	55½	sw.	sw.	calm		·03	
16.	30·122	29·981	29·960	29·93	29·87	71	52	62	55½	55	sw.	s.	ese.			·51
☾ 17.	30·058	29·965	29·956	29·89	29·92	71	48	66·5	57½	57	sw.	ssw.	calm	·02	·21	·03
18.	30·012	29·979	29·950	29·94	29·89	76	46	68	63	55	sw.	calm	ese.			
19.	29·980	29·960	29·945	29·75	29·73	72	43	68	55	55	sw.	s.	sse.			
20.	30·084	30·066	29·960	29·91	29·97	80	48	70	63½	57½	sw.	wnw.	ese.			
21.	30·173	30·148	29·967	30·02	29·99	83	44	64	58	55	sw.	calm	ssw.			·10
22.	30·204	30·163	29·963	29·68	29·94	84	45	67·5	58	55	s.	ssw.	wsw.			·08
23.	30·159	30·052	29·960	29·95	30·00	89	56	70	60½	55	se.	calm	w.			
24.	30·103	30·051	29·960	30·09	30·09	85	57	70	59	55	e.	ne.	nw.			
● 25.	30·054	30·040	29·954	30·11	30·20	89	58	70	55	55	e.	nnw.	nw.	·52		·03
26.	30·103	30·052	29·965	30·21	30·22	75	55	71	55½	54½	se.	ne.	n.	·02		·04
27.	30·152	30·078	29·965	30·22	30·25	67	51	63	56½	52½	se.	ne.	ese.			
28.	30·200	30·182	29·976	30·26	30·18	71	38	66	56½	52½	s.	nne.	calm			
29.	30·183	30·145	29·974	30·11	29·98	76	54	66	59	57	s.	calm	ese.			
30.	29·969	29·816	29·950	29·85	29·77	82	56	73	59½	55	sw.	sse.	ese.	·46		
31.	29·803	29·654	29·927	29·67	29·71	76	59	72	57½	56		sw.	e.	·36		
Mean.	29·964	29·886	29·946	29·871	29·884	73·19	50·00	64·8	56·43	54·08				2·40	1·24	2·52

XXXIII. *On the double Refraction temporarily produced in Isotropic Bodies.* *By* M. G. WERTHEIM*.

[With a Plate.]

Description of the Apparatus and manner of operation.

IN a homogeneous body submitted to compression in one direction only, the molecules are caused to approach each other in this direction, and they are forced asunder in the two perpendicular directions: the reverse takes place when we substitute traction for compression. The relations between simultaneous dilatations and compressions, have been actually ascertained to a degree of exactitude equal to that of which experimental questions are usually susceptible. This mechanical effect is accompanied by an optical one, which we can observe in transparent bodies, and which is explained by the diminution of the elasticity of the æther in the direction of the mechanical compression, and its augmentation in the direction of dilatation.

It is proposed to discover the relations subsisting between the force applied to a body of known dimensions and the two species of effects produced, whatever may be the mechanical or optical elasticities of the bodies operated on, provided that they are constant in all directions. In other words, the two surfaces of elasticity being spheres in the natural state of the body, it is proposed to determine the form and position of the two surfaces after the application of an external force, which, by pressure or by traction, acts in a uniform manner upon the whole body in one of its dimensions.

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* Extracted from a separate Pamphlet published by Mallet-Bachelier. Paris, 1854.

The apparatus (Plate II. figs. 1 and 2) which I have used to produce the mechanical effect is composed of two pieces for compression A_1 and A_2, of one piece for traction B, and two electromagnetic spirals, which form part of the apparatus of M. Ruhmkorff, and which carry, one the polarizing Nichol C, and the other the doubly refracting prism D. All these pieces are fixed upon a support of oak, furnished with notches so as to give passage to the hooks destined to bear the weights, and with grooves q in which slide the prolongations of the cores of the spirals.

It is easy to understand the dispositions of the presses which we shall name *weight presses*, fig. A ; each of them is composed of a vertical stirrup a, fastened down to the bench, and of the two horizontal plates b, parallel to each other and to the horizontal portion of the stirrup. This latter is pierced with two apertures, in which slide with gentle friction two cylindrical stems c, which connect the two moveable plates, the upper one of which is placed on the piece p submitted to compression, while the lower one is furnished with a ring d for the application of the charge. The whole press is in bronze, 2 centimetres thick and 3 centimetres wide, and hence sufficiently solid to prevent any of its parts from suffering a sensible flexure under the action of the charge, which never exceeded 600 kilogrammes. The parallelopiped p is placed on the horizontal portion of the stirrup, so that its centre is in the prolongation of the axes of the prisms ; the two horizontal surfaces of this piece, those that receive the pressure, are furnished each with a plate of vulcanized india-rubber, and several doubles of thin pasteboard. In this manner the pressure distributes itself in a uniform manner over the whole parallelopiped, since the upper plate cannot deviate from the horizontal line ; and if, notwithstanding these precautions, some defect of parallelism should exhibit itself, it can always be remedied by means of the screws r.

One of these weight presses A_1 is destined for the employment of heavy charges, which are placed in the chest E furnished with a support at its base ; it is only necessary to lower this support to cause the entire charge to be carried by the bar e, which is suspended from the ring d.

The other press A_2, the use of which we shall explain immediately, is destined to receive comparatively light charges only ; it therefore carries only a square bar f, furnished with cross-bars at its two extremities, which reach over the bench, and receive each one-half of the charge.

The apparatus for extension, fig. B, is composed of a support of cast iron g, larger and more elevated than the stirrups of the presses, and of the two truncated bronze cones, the superior one of which carries a strong vice i, while the inferior one k is fur-

nished with a ring *l,* destined to carry the hook *m* and the weight-chest E. The piece *p,* which is submitted to extension, is fixed with red mastic to the bases of these two cones, which has the double inconvenience of rendering the experiment deli-cate, since the mastic is detached by the least transverse shock, and of limiting its extent. This mastic scarcely supports, for the square centimetre, a traction of 50 kilogrammes. However, this mode of fixation is the only one admissible; for the vice, or any other mechanical appliance, necessarily exerts unequal pressures in different directions, which give rise to the exhibition of colours of the same nature as those we would observe, but so irregularly distributed as completely to conceal the principal phænomenon. The vice *i* of the superior cone is maintained vertical by means of the traverse *n,* and its screw *o* serves to place the centre of the piece *p* in the axis of the apparatus.

These three supports are usually fixed on the bench; how-ever, when it is desired to bring into simultaneous action both mechanical and magnetical forces, two of the supports are raised so as to leave only the piece A or the piece B, according as we operate by pressure or by traction.

In causing the spirals to glide in their grooves *q,* they are made to approach as nearly as possible to the piece to be sub-mitted to their action; they are fixed in this position by means of the screws F, and against their bases the two pieces of soft iron *s* are applied so that their cores constitute a single electro-magnet. It is doubtless unnecessary to remark that an interval ought always to exist between the piece *p* and the poles, suffi-ciently large to prevent the poles from moving towards each other when the current is excited, and clasping the piece between them, which would introduce a mechanical compression in the direction of the axis of the piece. The same motive has caused me to reject the iron and steel presses which I had first employed.

In the prolongation of the optic axis of the apparatus is placed a plate of white Sèvres porcelain G, strongly illuminated by white light, by the light of the salt-lamp, or else by a Carcel lamp, the rays of which, before arriving at the Nichol, have tra-versed glasses or liquids sensibly monochromatic. It would doubtless be better to operate with direct white light, and with coloured rays well defined by their vicinity to a line of the spec-trum; but this has not been possible, owing to the situation of my laboratory and the long duration of each experiment.

Supposing now, to fix the ideas, that a cube of a transparent substance, colourless and isotropic, is placed in the press, that the principal sections of the Nichol and of the doubly refracting prism are parallel to each other, and enclose an angle of 45° with the vertical, and that white light is made use of. As long as

R 2

the weight-chest reposes on its support, two images of the aperture of the diaphragm placed behind the Nichol, a white or ordinary image, and a black or extraordinary one, are seen. When the cube is of glass, a gray tint is perceived on the latter image; but this effect, due to a residue of temper, may usually be neglected in the case of well-annealed glasses, as we shall see further on.

As the pressure augments, the two images become successively coloured with all the colours of thin plates; the ordinary image always taking the colours of transmitted light, and the extraordinary image presenting the complementary tints, or the colours of the rings in reflected light.

The two images exhibit flat tints, if the experiment is well made, when the pressure is exerted in a uniform manner, and the substance is very homogeneous; but if the least irregularity exists, it is discovered by the experiment itself, and this, I think, renders this method preferable to any purely mechanical means. Thus, when the upper plane b deviates in a slight degree from the horizontal line, and when consequently the charge is thrown more upon one of the edges of the parallelopiped, we see at the same side, in each image, a colour more elevated in the scale than on the side opposite. This irregularity usually indicates that the piece is not exactly in the middle of the press, at least that the two horizontal faces of the piece possess some slight defect of parallelism.

When the piece, at the same time that it is compressed, takes a transverse flexure,—which is often the case, notwithstanding their small height,—however small this flexure may be, coloured bands make their appearance similar to those obtained by means of the press of Fresnel : these bands sometimes vanish by a slight displacement, or by the turning of the piece; but in many cases it is necessary to cut the faces anew, or to reject the piece altogether. Finally, different colours are sometimes observed in the two images which are quite irregularly distributed, and which are a certain indication that some defect of homogeneity exists in the substance.

Method employed to measure the artificial double Refraction.

We have already said that the colours follow the series of tints in coloured rings. Newton having measured the thickness of the plate of air which transmits and reflects each of these complementary colours, it is natural to attempt to make use of these measurements to express numerically each tint obtained either by compression or by traction. The following table contains some of these determinations; the thickness of the plate of air e is expressed in millionths of the English inch, and the

charge P in kilogrammes. This latter acts as a pressure, and the weight of the chest E, of the hook e and the press A, are comprised in the charges stated in the table.

Colour of the image E.	e.	Crown glass.		Plate glass.		Crown glass of Clichy.		Flint glass.		Alum.	
		P.	$\frac{P}{e}$.	P.	$\frac{P}{e}$.	P.	$\frac{P}{e}$.	P.	$\frac{P}{e}$.	P.	$\frac{P}{e}$.
Very black	$\frac{1}{2}$										
Black	1	15	15·0	40	40·0	10	10·0	30	30·0	30	30·0
Bluish	2	32	16·0	90	45·0	25	12·5	58	29·0	60	30·0
Blue...............	$2\frac{2}{5}$	40	16·6	100	41·7	32	13·3	80	33·3	80	33·3
White	$5\frac{1}{4}$	72	13·7	210	40·0	66	12·6	145	27·6	150	28·6
Yellow	$7\frac{1}{9}$	100	14·1	300	42·2	90	12·7	175	24·6	225	31·6
Orange	8	130	16·3	370	46·2	110	13·8	230	28·8	248	31·0
Red	9	140	15·5	420	46·7	138	15·3	257	28·6	300	33·3
Violet	$11\frac{1}{6}$	155	13·9	480	42·9	144	12·9	295	26·4	352	31·5
Indigo	$12\frac{5}{8}$	170	13·2	520	40·5	150	11·7	320	25·0	410	32·0
Blue...............	14	190	13·6	560	40·0	184	13·1	360	25·7	450	32·1
Green	$15\frac{1}{8}$	212	14·0	...	...	205	13·6	420	27·8	492	32·5
Yellow	$16\frac{2}{3}$	240	14·7	...	...	242	14·9	475	29·1		
Orange............	$17\frac{2}{3}$	260	15·1	...	...	251	14·6	515	29·9		
Bright red	$18\frac{1}{3}$	287	15·7	...	...	280	15·3	550	30·0		
Deep red	$19\frac{2}{3}$	290	14·7	...	...	295	15·0				
Purple	21	300	14·3	...	...	310	14·8				
Indigo	$22\frac{1}{10}$	310	14·0	...	...	320	14·5				
Blue...............	$23\frac{2}{3}$	325	13·9	...	...	334	14·3				
Green	$25\frac{1}{8}$	370	14·7	...	...	370	14·7				
Yellow	$27\frac{1}{4}$	395	14·6	...	...	391	14·4				
Orange............	29	430	14·8	...	...	423	14·6				
Bluish-red	32	465	14·5	...	...	466	14·6				

It is evident that the differences of the paths of the two rays are sensibly proportional to the charges, still the variations which subsist for the same piece in the values of the ratio $\frac{P}{e}$ sufficiently indicate that the procedure is not capable of furnishing very exact results.

In point of fact, each of the colours exhibits a great number of degradations, and however careful we may be to choose the most decided tint, and that which accords best with the designation of Newton, it is hardly reasonable to hope that we shall

alight on the exact tint to which his measurement of the thickness of the plates refers.

To operate with more exactitude, I have had recourse to the homogeneous light emitted by salt and alcohol, which light, according to Mr. Miller, contains almost solely orange rays in the vicinity of the line D of Fraunhofer, and the wave-length of which is consequently 589 millionths of a millimetre. We shall hence have extinction of the extraordinary image whenever the difference of the paths is an even multiple of $\frac{\lambda_0}{2}$ or of 294·5 ; and extinction of the ordinary image whenever the difference of path amounts to an odd multiple of the same quantity; we shall also know the amount of the charges which produce this difference of path. It must, however, be remarked, that besides the orange rays, the flame of alcohol and salt contains a certain quantity of violet light; hence in these experiments the black colour, which would be the result of a complete extinction, is replaced by a violet coloration, which is very distinct in one of the two images, when all the orange rays have disappeared; this, however, does not interfere in the slightest degree with the exactitude of the experiments, for it is easy to seize the moment when this colour presents itself in the greatest purity and with the least intensity. In this manner we obtain a certain number of fixed points in the chromatic scale, which it is our purpose to construct, and others are found with equal certainty by operating with white light.

M. Biot[*] assigns the arc of 24 degrees to the rotation impressed upon the mean yellow rays by a plate of rock-crystal of the thickness of one millimetre; combining this determination with the rotations which refer to the lines of the spectrum, the length of whose undulations are known, we find for these rays the length 550·6, and consequently we have $\frac{\lambda_y}{2} = 275\cdot3$; but we also know the limiting tints of the reflected and transmitted rings; they are those in which the mean yellow ray is destroyed. We can therefore determine the charges which give these *teintes de passage,* sufficiently sombre in the first rings at least, and characterized with sufficient clearness to serve as points of reference. Among these colours the *teinte sensible* distinguishes itself, which, according to M. Biot[†], is reflected by a plate of air of about 21 millionths of an English inch, or of

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[*] " Mémoire sur les phénomènes rotatoires opérés dans le cristal de roche" (*Mém. de l'Acad.* vol. xx. p. 284).

[†] " Mémoire sur la polarisation lamellaire " (*Mém. de l'Acad.* vol. xviii. p. 544).

533·4 millionths of a millimetre, which thickness is very nearly equal to $2\frac{\lambda_y}{2}$.

To construct the following table, I have chosen a piece of very pure Clichy crown glass (borosilicate of zinc of the manufacture of MM. Maës and Clémandot), which could be compressed in the press A_1 until it gave a difference of path of $7\frac{\lambda_y}{2}$; beyond this limit the colours are too much mixed with white light, and are consequently too pale to serve as measures. The charges which it was found necessary to employ are as follows:—

	$\frac{\lambda}{2}$.	$2\frac{\lambda}{2}$.	Diff.	$3\frac{\lambda}{3}$.	Diff.	$4\frac{\lambda}{2}$.	Diff.	$5\frac{\lambda}{2}$.	Diff.	$6\frac{\lambda}{2}$.	Diff.	$7\frac{\lambda}{2}$.	Diff.
With white light ...	68	142	74	216	74	290	74	365	75	440	75	515	75
With the flame of salt and alcohol.	74	153	79	233	80	313	80	394	81	475	81		

These figures are the means of a great number of concordant measures; they may be considered exact to about 1 kilogramme for the experiments with the white light, and to 2 kilogrammes for the experiments with the homogeneous orange light.

The charge which produces the difference of path $\frac{\lambda}{2}$ is considerably less than all which follow; this same inequality is presented by all isotropic bodies, and we shall have to take it into account; but at present it is enough to prove that it is sufficiently small, and that the augmentation of the charge is sufficiently regular, to permit of our finding by interpolation the difference of path corresponding to any charge, and consequently to any tint whatever. We have chosen in each ring a certain number of tints well characterized and easy to reproduce, and have determined the charges necessary to be applied to the same piece of crown glass to produce them. Let P_x be one of the charges, and suppose that it stands between P_a which gives the double refraction measured by $n\frac{\lambda_0}{2}$, and P_b which corresponds to $(n+1)\frac{\lambda_y}{2}$; we have for this charge the difference of path

$$x = n\frac{\lambda_0}{2} + \left[(n+1)\frac{\lambda_y}{2} - n\frac{\lambda_0}{2}\right]\frac{P_x - P_a}{P_b - P_a}.$$

This formula has served for the construction of the following table, in which the second column d contains the difference between the paths traversed by the ordinary and extraordinary rays, expressed in millionths of a millimetre.

In the third column e, are inscribed the thicknesses of the plates of air which transmit and reflect the colours inscribed in

the fifth and sixth columns. These thicknesses are calculated in millionths of the English inch, so that they may be immediately compared with the measures of Newton.

Finally, we shall see that the designations of the colours, which I have sought to render as faithful as possible, agree most usually with those recently published by M. Brücke*.

No.	d in millionths of a millimetre.	e in millionths of an English inch.	P charge.	Colour of the O image.	Colour of the E image.
	mm				
1	0	0	0	White.	Black.
2	40	0·78	10	White.	Iron-gray.
3	97	1·91	24	Yellowish-white.	Lavender-gray.
4	158	3·11	39	Brownish-white.	Blue-gray.
5	218	4·29	54	Yellow-brown.	A clearer gray.
6	234	4·61	58	Brown.	White with a slight tint of green.
7	259	5·10	64	Clear red.	White, almost pure.
8	267	5·25	66	Carmine-red.	Yellowish-white.
9	275	5·41	68	Brown-red very deep, almost black.	Pale yellow.
10	281	5·53	70	Deep violet.	Pale yellow.
11	306	6·02	77	Indigo.	Clear yellow.
12	332	6·53	84	Blue.	Brilliant yellow.
13	430	8·46	110	Greenish-blue.	Yellow-orange.
14	505	9·84	130	Bluish-green.	Reddish-orange.
15	536	10·55	138	Pale green.	Warm red.
16	551	10·84	142	Yellowish-green.	Deeper red.
17	565	11·12	146	Green, clearer.	Purple.
18	575	11·32	149	Greenish-yellow.	Violet.
19	589	11·59	153	Vivid yellow.	Indigo.
20	664	13·07	183	Orange.	Blue.
21	728	14·33	200	Brownish-orange.	Greenish-blue.
22	747	14·70	205	Carmine-red, clear.	Green.
23	826	16·26	216	Purple.	Clearer green.
24	843	16·59	221	Violet-purple.	Yellowish-green.
25	866	17·05	228	Violet.	Greenish-yellow.
26	910	17·91	240	Indigo.	Pure yellow.
27	948	18·66	250	Deep blue.	Orange.
28	998	19·64	263	Greenish-blue.	Reddish-orange, vivid.
29	1101	21·67	290	Green.	Violet-red, deep.
30	1128	22·20	298	Yellowish-green.	Bluish-violet, clear, teinte de passage.
31	1151	22·66	305	Impure green.	Indigo.
32	1258	24·76	334	Flesh-colour.	Blue, greenish tint.
33	1334	26·26	354	Moderate red.	Bluish-green (water-
34	1376	27·09	365	Violet.	Brilliant green. [green).
35	1426	28·07	380	Violet-blue, grayish.	Greenish-yellow.
36	1495	29·43	400	Greenish-blue.	Rose-red.
37	1534	30·20	410	Fine green.	Carmine-red.
38	1621	31·91	432	Clear green.	Purple-carmine.
39	1652	32·52	440	Yellowish-green.	Violet-gray.
40	1682	33·11	449	Greenish-yellow.	Gray-blue.
41	1711	33·68	458	Gray-yellow.	Greenish-blue, clear.
42	1744	34·33	468	Mallow.	Bluish-green.
43	1811	35·65	486	Carmine.	Fine clear green.
44	1927	37·93	515	Gray-red.	Clear gray-green.
45	2007	39·51	535	Gray-blue.	Gray, almost white.

This table has served for all the experiments with white light; the most sensible tints have been preferred, those which are found on the limits of the rings.

But it is not always possible to push the experiments to so great a difference of path, partly because of the two great dimensions of the piece, and partly because the substance will not support a notable compression without being bruised, cloven, or at least without being optically changed. This last alteration is observed in several crystallized bodies, and among others in rock-salt. It consists in a separation of the laminæ of the crystal, which, though invisible to the eye, manifests itself in polarized light by permanent chromatic phænomena, of such an intensity that they often predominate over the temporary effect of the charge*.

In the experiments by means of traction, we are equally obliged to stop at small charges on account of the fragility of the mastic; it was therefore indispensable to be able to measure small differences of path with a precision greater than that which appertains to the determinations made by means of the pale and not very sensible tints which are found at the commencement of the first ring.

This object has been attained by the simultaneous employment of two pressures, or of one pressure and one traction. We place under the press A_1, either the piece of crown glass which we have just employed, or another piece for which the charges producing the colours inscribed in our table have been previously determined, and compress it until the *teinte sensible* appears. We then place the piece to be examined either in the press A_2, or in the apparatus of extension B, and apply to it the feeble charge which it can carry without inconvenience. Looking now across the two pieces, we see that the *teinte sensible* has disappeared, and that it is replaced by a colour, more elevated, if the second piece be compressed, and by a colour inferior in the order of tints if the piece be elongated. In order to produce the *teinte sensible*, it is necessary, in the first case, to remove from the press A_1 a charge *equivalent* to that which has been applied to A_2, and, in the second case, to add a charge equivalent to that which acts on B. We see that the press A_1 then acts as a veritable compensator, the sensibility of which may be regulated at pleasure by placing in it parallelopipeds which require charges more or less considerable.

We may operate in a different manner by comparing the traction with the pressure; first, we apply the traction, and afterwards seek the charge which it is necessary to apply to A_1 in order to reduce the difference of the paths to zero. This

* *Comptes Rendus,* vol. xxxiii. p. 577.

method is more direct than the preceding one; but it often leaves more uncertainty, because of the invariably incomplete extinction of the image E.

Suppose, in the first place, that two compressions had been combined.

Let P_a be the charge which it is necessary to apply to the compensating piece placed on A_1, so that the double refraction ascends from $3\dfrac{\lambda_y}{2}$ to $4\dfrac{\lambda_y}{2}$;

P_1 and P_2 two equivalent weights, the first of which has been removed from the press A_1, and the second placed upon the press A_2;

P_b the weight sought, which produces in the piece placed on A_2 the double refraction measured by $\dfrac{\lambda_y}{2}$;

then we have

$$P_b = \frac{P_2}{P_1} P_a.$$

This formula applies equally to the traction; only if we operate according to the second method, which is that of the complete compensation of two double refractions, P_a represents the charge which produces, by pressure, the difference of path $\dfrac{\lambda_y}{2}$.

These compensations also furnish us with a means of verifying the exactitude of an hypothesis which we have implicitly admitted in the construction of our table. After having demonstrated that the differences of path which correspond to all charges may be found by means of interpolations between the fixed points $\dfrac{\lambda_y}{2}, \dfrac{\lambda_0}{2}, 2\dfrac{\lambda_y}{2}, 2\dfrac{\lambda_0}{2} \ldots$, we have afterwards made the same interpolations between 0 and $\dfrac{\lambda_y}{2}$; but this latter we were not authorized to do, and the accuracy of the procedure has not thus far been verified, save by the coincidence of our determinations with the measurements of Newton.

We can now fill this gap; for this purpose we place in the two presses two pieces, for which the values P_a and P_b have been previously determined by direct experiment. We compress the former up to the *teinte sensible,* and we determine afterwards the equivalent charges P_1 and P_2 which correspond to the different double refractions comprised between 0 and $\dfrac{\lambda_y}{2}$; if between these two limits the optic compressibility is subject to any notable

variation; if, for example, it be greater between 0 and $\frac{\lambda}{4}$ than it is between $\frac{\lambda}{4}$ and $\frac{\lambda}{2}$, this variation will manifest itself in the successive values of P_b which are found by means of our formula. If, on the contrary, the proportionality exists between these two limits, we shall obtain, by calculation, the same value of P_b that has been found by direct compression.

The following table contains some of these determinations :—

	By direct measurement.		Equivalent weights.		Calculated.
	P_a.	P_b.	P_1.	P_2.	P_b.
Two compressions, flint and crown......................	140	68	32	15·0	65·0
	...	...	58	28·0	67·0
	...	...	90	45·0	70·0
	...	...	120	60·0	70·0
Two compressions, plate glass and crown	292	185	50	31·0	181·0
	...	...	80	50·0	182·0
	...	...	120	87·0	187·0
Two compressions, crown and alum	75	27	14	5·0	26·8
	...	...	34	12·5	27·6
	...	...	71	25·0	26·4
	...	...	120	45·0	28·1
Compression and traction, crown and plate glass ...	68	210	12	35·0	198·0
	...		19	55·0	197·0
Compression and traction, crown and flint	68	155	16	35·0	149·0
	...	...	24	55·0	156·0
	...	...	33	75·0	156·0
	...	...	44	95·0	147·0
	...	...	52	115·0	150·0

These examples are sufficient to enable us to form a judgement as to the exactitude of the procedure; they show us at the same time the little variations of compressibility and extensibility which occur within the limits of the first half ring, and which we are to take into account in the construction of the curve which represents the route of the compressions and elongations.

After the details into which I have just entered, I can in the subsequent portion of this memoir dispense with describing for each piece the whole series of operations to which it has been submitted, and confine myself to stating the charges which, by traction or pressure, have successively produced differences equal to $\frac{\lambda}{2}$, or to the length of a semi-undulation of the mean yellow ray. For example, I will designate by $T_2 \frac{\lambda}{2}$ and $P_2 \frac{\lambda}{2}$ the weights which it is necessary to add, so that the difference of path which was $\frac{\lambda}{2}$ may be equal to $2\frac{\lambda}{2}$, and so of the rest; consequently the total charge will always be equal to the sum of the successive values of T and P.

General Results of the Experiments.

Substances.	No. of piece.	Height. H. (mm)	Length. Lo. (mm)	Breadth. La. (mm)	Traction $T_{2\lambda/2}$	Traction $T_{1\lambda/2}$	Pressure $P_{1\lambda/2}$	$P_{2\lambda/2}$	$P_{3\lambda/2}$	$P_{4\lambda/2}$	$P_{5\lambda/2}$	$P_{6\lambda/2}$	$P_{7\lambda/2}$	$\dfrac{P_{1\lambda/2}}{La}$	Mean for each substance.	Coefficient of optic elasticity.
Plate glass	1	23·7	24·8	26·0		220	212	220						8·2		
		26·0		23·7		210	197	203						8·3	8·25	32396
	2	23·7	22·5	26·0	205	210	217	225						8·3		
		26·0		23·7			195	203	206					8·2		
Common crown	3	26·2	26·2	26·2		267	225	225						8·6		
	4	19·0	25·8	26·2			250	259						9·5		
	5	19·0	28·0	25·9			206	210						7·9	8·84	34714
	6	28·5	27·4	27·7			250	257						9·0		
	7	10·0	29·4	10·6			98	102	104	104				9·2		
Crown of the manufacture of MM. Maës and Clémandot (borosilicate of potash and zinc).	8	20·0	29·3	9·7		74	68	74	74	74				7·0		
	9	9·7		20·0		150	130	130	132	138	75	75	75	6·5	6·87	26978
		9·8	29·3	11·0		85	78	80	76	81				7·1		
Crown of the manufacture of M. Feil (Guinand) (borosilicate of potash and zinc)	10	19·7	35·0	21·5			160	165	164					7·4		
	11	21·5		19·7			155	158	158					7·9		
		17·9	35·0	22·3	188	207	151	158	157					6·8	7·62	29923
		22·3		17·9			150	153	157					8·4		
Flint of MM. Maës and Clémandot	12	7·3	27·3	19·2		180	160	155	165					8·3	8·30	32593
Flint of Guinand	13	23·3	30·5	30·5	151	155	190	190		146				6·2		
	14	20·3	30·0	19·9			140	146	145					7·0	6·60	25917
Heavy flint of the manufacture of M. Feil.	15	24·5	30·0	30·0		185	420							14·0		
	16	16·6	30·1	15·5			184	186						11·9	12·97	50932
	17	16·9	17·9	17·9			232	240						13·0		

General Results of the Experiments.

Traction columns ($T_{2\lambda/2}$, $T_{1\lambda/2}$) are blank (…) for every row. The letter **k** appears at the head of each pressure column.

Substances.	No. of piece.	Height H. (mm)	Length Lo. (mm)	Breadth La. (mm)	$T_{2\lambda/2}$	$T_{1\lambda/2}$	$P_{1\lambda/2}$	$P_{2\lambda/2}$	$P_{3\lambda/2}$	$P_{4\lambda/2}$	$P_{5\lambda/2}$	$P_{6\lambda/2}$	$P_{7\lambda/2}$	$\dfrac{P_{1\lambda/2}}{La}$	Mean for each substance.	Coeff. of optic elasticity.
Faraday's flint………	18	18·0	18·0	18·0	…	…	263·0	270·0	…	…	…	…	…	14·6	14·60	57332
Borosilicate of lead of the manufacture of M. Feil …	19	12·5	27·2	13·7	…	…	179·0	187·0	…	…	…	…	…	13·1	13·25	52031
White from England……	20	13·7	…	12·5	…	…	168·0	172·0	173	…	…	…	…	13·4		
Clear violet of Derbyshire.	21	5·3	8·7	8·7	…	…	109·0	114·0	117	…	…	…	…	12·5	14·16	55605
„ „	…	8·7	14·4	5·3	…	…	66·0	68·0	68	70	70	…	…	12·5		
Dichroite from Beer Alston	22	9·3	9·4	9·4	…	…	136·0	…	…	…	…	…	…	14·5		
Yellow from Saxony ……	23	…	9·5	14·4	…	…	220·0	…	…	…	…	…	…	15·3		
Green from Siberia ……	24	20·8	14·2	9·4	…	…	136·0	…	…	…	…	…	…	14·5		
„	25	14·2	20·4	14·2	…	…	200·0	…	…	…	…	…	…	14·1		
„	…	17·5	20·8	14·3	…	…	224·0	…	…	…	…	…	…	15·7		
Ammoniacal alum, inactive …	26	14·0	20·8	13·0	…	…	45·0	…	…	…	…	…	…	3·5	3·87	15197
„	…	20·8	14·4	13·0	…	…	54·0	…	…	…	…	…	…	3·9		
„	…	9·5	9·1	9·1	…	…	42·0	…	…	…	…	…	…	4·6		
„	…	…	…	14·4	…	…	63·0	…	…	…	…	…	…	4·4		
„	…	15·4	12·2	11·4	…	…	36·5	39·5	40	43	44	…	…	3·2		
„	…	12·3	17·5	7·5	…	…	27·0	27·0	28	29	28	29	…	3·6		
From Dieuze ………	27	17·8	18·3	18·2	…	…	180·0	…	…	…	…	…	…	9·9	9·80	38483
From England ………	28	21·8	20·6	21·8	…	…	224·0	…	…	…	…	…	…	10·3		
From Wielizka ………	29	16·2	25·8	15·9	…	…	160·0	…	…	…	…	…	…	10·1		
„ „	30	31·5	19·9	16·4	…	…	160·0	…	…	…	…	…	…	9·8		
„ „	31	19·3	19·3	19·3	…	…	171·0	…	…	…	…	…	…	8·9		

Left-margin category labels: **Fluor-spar** (Clear violet of Derbyshire … Green from Siberia), **Rock-salt** (From Dieuze … From Wielizka).

All these substances are colourless, with the exception of the heavy flints and some of the fluorides of lime, and among these latter there are some possessing a tint so clear that the light, after having traversed a considerable distance of them, might still be regarded as sensibly white; we have sought to neutralize the tinting of the others by glasses coloured with the complementary tint. Another process consists in employing in the first instance the substance itself as a coloured glass, and determining by the compression of a colourless glass the lengths of the undulations of the light which it transmits. We shall see immediately, that when once this length is known, it is easy to calculate the weight which it would have been necessary to employ with white light.

I have called *inactive alum* an alum procured from a manufactory at Muirancourt (Aisne), respecting the method of preparation of which I have been unable to obtain any information; very limpid, not tarnishing in moist air, and preserving for years the polish imparted to its surfaces, natural or artificial; this alum possesses also the precious quality of being completely exempt from laminar polarization, notwithstanding the notable quantity of ammonia which enters into its composition. Even when a piece of this alum is pressed to crushing, the fragments exert no action on polarized light; it also distinguishes itself from ordinary alum by its conchoidal fracture.

The numbers which refer to rock-salt do not merit the same degree of confidence as the other numbers contained in the preceding table; notwithstanding all the care which I have taken to neutralize the pre-existent laminar polarization, and the permanent tinting which exhibits itself on the application of the mechanical force itself, this cause of error, combined with the imperfect transparency of the substance, has necessarily rendered the determinations less sure and precise.

I have cited in the foregoing table, among the crystals which belong to the regular system, only the species and the individual (if I may use the expression) which under the action of mechanical forces behave as homogeneous or isotropic bodies[*].

Experimental Laws.

1. *The weight (necessary to be applied to produce a certain double refraction) is independent of the height of the parallelopiped.*

This proposition, evident of itself, is confirmed by all the experiments; to cite only one example, the value of $P_{1\frac{\lambda}{2}}$ is

the same for the two specimens of fluor-spar 21 (first experi-

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[*] See my notes on the artificial double refraction produced in crystals of the regular system (*Comptes Rendus*, vol. xxxiii. p. 577, and vol. xxxv. p. 276).

ment) and 22, although their heights are in the proportion of 1 to 2.

2. *The weight is independent of the length of the piece.*

This law is demonstrated by the equality of weights applied to the pieces 1 and 2, 21 and 23, 31 and 32, &c., and it is confirmed by the direct experiment of verification which follows. After having determined for the piece 10 the value of P, I cut the piece in two by a saw perpendicular to its length; two pieces were thus obtained of the same transverse dimensions as the entire piece, but the lengths of which were 10·5 millims. and 22 millims.; each of these two parts, tested in both directions, required, to give the same tint as the entire piece of 35 millims., the application of the same charge as we have found for the latter.

3. *For the same substance, the charges are proportional to the width of the pieces.*

We have verified this law by means of the charge $P_{1\frac{\lambda}{2}}$, which

has been determined for all the pieces; in fact, the fifteenth

column of our table contains the values of the ratio $\dfrac{P_{1\frac{\lambda}{2}}}{La}$, and these

values are constant for the same substance, excepting such differences as may be attributed to slight variations in their density or structure.

But the pieces 10 and 11 present a remarkable anomaly; both of them require greater charges when the smallest of their transverse horizontal dimensions is placed in the sense which we denominate as width. They furnish, therefore, for the ratio, values which differ among themselves, according to the direction in which the pressure has been applied.

It must be remarked that these two parallelopipeds, as also the piece 12, have been obtained by a process called *refoulement,* and which consists in compressing the glass while it is still in the condition of a paste, so as to make it take the form of the heated mould. The annealing is not always sufficient to cause the traces of this operation to disappear; hence it is that this glass, perfectly homogeneous in appearance, and which in polarized light shows only the gray cross which is attributed to its temper, is already, and in a permanent manner, compressed in one direction and dilated in the two others.

Knowing the direction in which the pasty mass has been compressed, I have been able to convince myself, by these experiments and by others not included in the table, that the greatest charge always corresponds to the pressure applied in the

direction of *refoulement,* that is to say, in the direction in which the molecules had been previously approximated.

It might be supposed that this difference ought not to be observed except at the commencement of the experiment, and that a slight traction applied in the direction of *refoulement,* or a slight pressure in the directions perpendicular to the latter, would be sufficient to restore the equilibrium, and to bring the glass to the state of homogeneity which constitutes the true zero of the experiment. But, far from this, the difference exists under the greatest pressures, which proves that there is an alteration in the homogeneity of the matter, which does not manifest itself by any optic phænomenon, as long as the body is not subjected anew to the action of mechanical forces.

This fact appears to me to be very important, not only because it is in opposition to the opinion generally admitted, according to which all defects of homogeneity are indicated by an action on polarized light, but also because the same anomaly is often met with in crystals, and can furnish us with indications regarding the molecular pressures which such a crystal must have experienced at the moment of its formation.

4. *The double refraction, or the difference of path between the two rays, is proportional to the mechanical lengthening or shortening, but these are not rigorously proportional to the charges. Taking for abscissæ the weights, and for ordinates the shortenings and lengthenings which these weights produce, we obtain for the pressures a curve concave to the abscissæ, and for the tractions a curve convex to the abscissæ; these curves become less as the charges increase, and finally confound themselves with the same straight line, which corresponds to the coefficient of elasticity generally adopted.*

Our last table shows the comparative route of the double refractions and the charges; the value of $P_{\frac{\lambda}{2}}$ is always the most feeble, the charges augment afterwards up to $P_{3\frac{\lambda}{2}}$ or $P_{4\frac{\lambda}{2}}$, and from this point forward seem to rest stationary. The inverse takes place with the tractions; the greatest values are those of $T_{\frac{\lambda}{2}}$; they diminish afterwards; and although the experiments have not been pursued as far as those of compression, they suffice to show that the two curves approximate to one and the same straight line.

Before going further, it will be necessary for us to resolve the following question :—Is there an absence of proportionality between the double refraction and the purely mechanical effect, or between the mechanical effect and the charge?

I have attributed to the latter the cause of the observed differences; these are my reasons : Mechanical force acts directly only upon ponderable matter; the differences in the tension of the æther are only a secondary consequence of the differences of the molecular or mechanical tensions. The double refraction is necessarily a function of the temporary linear changes, and, for the same substance, the optic axes depend for magnitude and position on the principal mechanical axes. It is therefore necessary that the form of this function should change according to the intensity of the mechanical forces applied externally, and that it should change not only with the amount of the charges, but also with their mode of action ; since the double refractions differ among themselves according as the substance is compressed or elongated by the same weight, provided the latter does not exceed a certain limit. In the first case, we produce artificially a body possessing negative double refraction, in which the extraordinary ray is propagated with greater velocity than the ordinary ; in the second case we obtain a positive body, in which it is the ordinary ray which moves quickest. If, now, with double refractions of different magnitudes in the two cases, the mechanical shortenings and lengthenings were still equal to each other, it would follow that the ratio of the indices of refraction of the two rays would vary according as the one or the other moves quickest ; which conclusion is hardly admissible.

It might be objected, it is true, that the double refraction does not depend upon the linear change which takes place in the direction of the force, but upon the difference between this latter and the changes of length which occur in the two directions perpendicular to that of the force, and that this difference, or what amounts to the same thing, that the law of change of volume might vary with the magnitude and the sign of the force applied ; but this would be a purely gratuitous hypothesis, without any fact to support it.

On the contrary, we find a decisive proof in favour of the explanation which we have given, in the results of the direct measurements of the elongations and compressions. It has often been observed, that the first elongations, which require the most feeble charges, are too small as compared with the subsequent elastic elongations. This remark has been made by M. Poncelet[*] with reference to the experiments of M. Ardant ; the same thing reproduces itself in my experiments and in those of Mr. Hodgkinson. On the contrary, the first compressions obtained by the same author are generally too great; only, inasmuch as they refer to differences so small as not to be capable of mea-

[*] *Mécanique Industrielle,* p. 347.

surement with certainty, being of the same order in point of magnitude as the possible errors, no conclusion can be drawn from them.

Our method causes all these uncertainties to disappear; we have already remarked, that with our parallelopipeds, which possess on an average a section of 500 square millimetres perpendicular to the direction of the force, a difference of 1 kilogramme in the force translates itself into a sensible difference of tint. Let us say, for the sake of more certainty, 2 kilogrammes; supposing the coefficient of elasticity of the substance to be only 5000, the result is that a difference in the charge of 4 grms. per square millimetre, and a shortening or elongation of less than a millionth of the height, are sensible and measurable, while such quantities completely escape the direct modes of experiment. We can even double or triple the sensibility of our process by doubling or trebling the length of the pieces; for with the same charge the tint remains the same, while the mechanical linear change will be only one-half or one-third of what it was. At the same time, we have the advantage of operating, not with bars several metres in length, but with small cubes, which enables us to avoid numerous causes of error.

Dividing the values of P and of T by the length La, I have reduced them to what they would be for a parallelopiped 1 millimetre in length, or what is the same thing (according to 1 and 2), to what they would be for a cube 1 millimetre the side; the weights have been plotted on the axis of the abscissæ, each division of which represents a kilogramme. The double refractions corresponding are represented by the ordinates, each division of which corresponds to a difference of path of $\frac{1}{20,000}$th of a millimetre in the air. It is to these curves which we must have recourse whenever we wish to determine with precision the lengthening or shortening which corresponds to a feeble charge; but we observe at the same time how little this curve deviates from the straight line which represents the ordinary coefficient of elasticity.

I have calculated, for all the pieces contained in the table, the ratios of the successive values of P and T. Assuming each value of $P_{,\frac{\lambda}{2}}$ equal to 10; here follow the means of these quotients*, which for the same difference of path differ very little from each other:

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* I have not comprised in this mean the tractions which refer to the pieces 10 and 12, and which show that, in the sense in which the pressures become too small, the tractions, on the contrary, augment in an extraordinary manner.

$T_{2\lambda},$	$T_{\lambda},$	$P_{\lambda},$	$P_{2\lambda},$	$P_{3\lambda},$	$P_{4\lambda},$	$P_{5\lambda},$	$P_{6\lambda},$	$P_{7\lambda}$
$\frac{}{2}$	$\frac{}{2}$	$\frac{}{2}$	$\frac{}{2}$	$\frac{}{2}$	$\frac{}{2}$	$\frac{}{2}$	$\frac{}{2}$	$\frac{}{2}$
10·64	10·85	10	10·34	10·42	10·75	11·01	10·88	11·02.

The latter compressions deduced from a small number of experiments are probably too great; we may assume that the first pressure $P_{1\lambda}$ is to the definite values of P and T as 1 is to 1·08;

consequently, starting from the point where the double refractions become proportional to the charges, we will multiply by this coefficient the numbers inscribed in the sixth column in order to find the constant augmentation of the charge.

We give the name of *coefficient of mechanical elasticity* E to the ratio between the charge applied to the unit of surface, and the elongation or compression which it has produced in the unit of length.

In a similar manner we will give the name of *coefficient of optical elasticity* C to the ratio between the charge applied to the unit of surface and the double refractions which it produces, taking for the unit of double refraction a difference of path in the air equal to the unit of length.

Let P be one of the mean values inscribed in the sixth column, we shall have for the coefficient of optic elasticity the following

expression, $C = P \times 1·08 \times \dfrac{1000000}{275}$; the different values of C

have been carried into the seventeenth column.

We might have represented by a formula of interpolation the route of the pressures and tractions; but it might have been demanded whether the proportionality really exists beyond a certain limit, whether the curve becomes confounded with a straight line in all the rest of its course, and whether this takes place even when permanent changes of length add themselves to the temporary changes.

The actual observations are still too limited to enable us to respond to these questions in an absolute manner; however, I believe that I have elsewhere demonstrated that the coefficient of mechanical elasticity increases and diminishes with the density of the substance. Now, according to the law of changes of volume, the compression augments the density, while the traction diminishes it; the curve thus appears to prolong itself at both sides to the two points of rupture, by tearing and by crushing.

The results which we have just obtained are of no practical importance in constructions; these differences are too small to be taken into account in the employment of materials, and our

experiments prove that we might safely continue to make use of the same coefficient of elasticity in calculating the effects of tractions and of compressions.

But the results acquire a great importance when they are regarded from the point of view of the theory of molecular forces, or those of mechanical oscillations, or of sonorous vibrations; I believe that they will furnish the solution of a certain number of questions which have hitherto rested in suspense, and to which I propose to return on another occasion.

5. *The difference* d *of the path passed over is independent of the length of the undulation* λ; *hence the dispersion of the double refraction is insensible.*

The charges P are proportional to the differences of path between the limits which we have defined; hence, if our proposition be true, we ought to have the equation

$$\frac{d}{d'} = \frac{P}{P'} = \frac{\lambda}{\lambda'},$$

in which λ and λ' are the lengths of the undulations of two simple rays, and P, P' the charges which must be applied to the same parallelopiped to obtain extinctions of the same order in the one or the other image.

This equation is already in part demonstrated by the determination of the fixed points which we have made use of in the construction of our table; to verify it between the widest limits, I thought it necessary to make some observations with the red and violet rays.

I have employed successively red glasses of two descriptions; experiment itself showed me that neither of the two was rigourously monochromatic; but in placing them one above the other, I have obtained a deep red colour without any trace of other rays. The violet light was obtained by the passage of white light through a solution of the ammoniacal sulphate of copper; the light transmitted still contained some yellow rays.

We shall now make use of our experiments to determine the colours of the spectrum, assuming for point of comparison the mean yellow ray $\lambda_y = 550$.

Substances.	No.		Violet.	White.	Salt and alcohol.	Red.	$\frac{\nu}{y}$.	$\frac{o}{y}$.	$\frac{r}{y}$.
			Weights applied with the light.				Ratios.		
Crown of Clichy.	8	$P_{1_{\frac{\lambda}{3}}}$.	52	68	74	78	0·765	1·088	1·147
		$P_{2_{\frac{\lambda}{2}}}$.	56	74	79	87	0·757	1·068	1·176
		$P_{3_{\frac{\lambda}{2}}}$.	58	74	80	89	0·784	1·081	1·203
		$P_{4_{\frac{\lambda}{2}}}$.	...	74	81	88	......	1·095	1·189
		$T_{1_{\frac{\lambda}{2}}}$.	128	155	...	...	0·826	......	......
Flint.............	14	$P_{1_{\frac{\lambda}{2}}}$.	107	140	145	182	0·764	1·036	1·300
		$P_{2_{\frac{\lambda}{2}}}$.	120	146	155	190	0·822	1·062	1·301
		$P_{3_{\frac{\lambda}{2}}}$.	120	145	167	189	0·828	1·152	1·303
Flint.............	13	$P_{1_{\frac{\lambda}{2}}}$.	...	185	...	222	......	......	1·200
		$P_{2_{\frac{\lambda}{2}}}$.	...	190	...	233	......	......	1·226
Mean	......	......	...	...	...	...	0·792	1·083	1·227

Consequently we have $\lambda_\nu = 436$; $\lambda_0 = 596$; $\lambda_r = 675$.

These numbers show that the solution of the sulphate of copper transmits rays of the line G, which is found at the limits of the indigo and violet; that the light of saline alcohol corresponds sensibly to the line D; and finally, that the two superposed red glasses transmit rays which are found in the spectrum far beyond the extreme red of Fresnel, without attaining, however, the extreme red of Fraunhofer.

<center>[To be continued.]</center>

XXXIV. *On some Extensions of Quaternions.* By Sir WILLIAM ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the French Institute, Hon. or Corr. Member of several other Scientific Societies in British and Foreign Countries, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

<center>[Continued from p. 137.]</center>

[17.] THE following remarks may be useful, as serving to illustrate and develope the general analysis con-

tained in some of the preceding paragraphs, especially in [14.], and as adapted to give some assistance towards any future study of associative polynomes, such as quines, of an order higher than quadrinomes, but subject like them to the law of conjugation (32).

The expression (98) may be thus more fully written:

$$(f) = (fee)^2 + (feg)(fge) + \Sigma'(feh)(fhe); \quad . \quad . \quad (112)$$

where *efgh* are all supposed to be unequal; the summation Σ' being performed relatively to *h*, for all those $n-3$ values of the latter, which are distinct from each of the three former indices. Interchanging *e* and *g*, and subtracting, we eliminate the symbol (f), and obtain the following formula:

$$\text{I.} \quad (fee)^2 - (fgg)^2 = \Sigma'\{(fgh)(fhg) - (feh)(fhe)\}; \quad (113)$$

which type I. includes generally $n(n-2)$ distinct and homogeneous equations, of the second dimension, with $2(n-2)$ terms in each, between the $\frac{1}{2}n^2(n-1)$ symbols of the form (fgh). Thus, for the case of *quadrinomials* $(n=3)$, by writing, in agreement with (82) and (83),

$$a_1 = (1), \ b_1 = (23), \ l_1 = (231), \ m_1 = (313), \ n_1 = (122), \quad (114)$$

and suppressing the sum Σ', we have by (112) the two expressions (compare a note to [12.]):

$$a_1 = m_1{}^2 - l_2 l_3 = n_1{}^2 - l_2 l_3; \quad . \quad . \quad . \quad (115)$$

together with four others formed from these, by cyclical permutation of the indices 1, 2, 3; and we are thus conducted, by elimination of the three symbols a_1, a_2, a_3, to three equations of the form $n_1{}^2 = m_1{}^2$; that is, to the 3 equations on the first line of (86), involving each 2 terms. For *quines* $(n=4)$, if we make also, with the same permitted permutations,

$$\left.\begin{array}{l} a_1 = (4), \quad c_1 = (14), \quad p_1 = (234), \\ r_1 = (141), \ s_1 = (142), \ t_1 = (143), \ u_1 = (144), \end{array}\right\} \quad (116)$$

the index *h* receives one value under each sign of summation Σ', and the resulting formulæ may be thus written:

$$(a_1 + l_3 l_2 + p_2 t_1 - s_1 p_3 =)n_1{}^2 + p_2 t_1 = m_1{}^2 - s_1 p_3 = u_1{}^2 + l_3 l_2; \quad (117)$$

$$(a_4 - s_1 t_2 - s_2 t_3 - s_3 t_1 =)r_1{}^2 - s_2 t_3 = r_2{}^2 - s_3 t_1 = r_3{}^2 - s_1 t_2; \quad (118)$$

where the line (117) is equivalent to three lines of the same form: so that the elimination of $a_1 .. a_4$ conducts here to 8 equations, of 4 terms each, between the 24 symbols of the form (fgh), or $l_1 .. u_3$, as by the general theory it ought to do. For polynomes of *higher* orders $(n > 4)$, we have the analogous equations,

$$\begin{aligned} (f) &- (feg)(fge) - (fgk)(fkg) - (fke)(fek) \\ &= (fee)^2 - (fgk)(fkg) + \Sigma'(feh)(fhe) \\ &= (fgg)^2 - (fke)(fek) + \Sigma''(fgh)(fhg) \\ &= (fkk)^2 - (feg)(fge) + \Sigma''(fkh)(fhk); \quad . \quad (119) \end{aligned}$$

where h, under Σ'', receives only $n-4$ values, being distinct from each of the four unequal indices, *efgk*.

[18.] By changing e to f in (95), and attending to the properties of the symbols (fgh), we obtain the expression

$$(fg) = \Sigma(fgh)(hff) ; \quad \ldots \quad \ldots \quad (120)$$

where f and g are unequal, and the summation Σ extends from $h=1$ to $h=n$. The term for which $h=f$ vanishes, and the formula (120) may be thus more fully written :

$$(fg) - (fge)(eff) = (fgg)(gff) + \Sigma'(fgh)(hff) ; \quad . \quad (121)$$

where the letters *efg* denote again some three unequal indices, and the summation Σ' is performed as in the foregoing paragraph. But also, by (97) and (100),

$$(fg) - (fge)(eff) = (fee)(gee) + \Sigma'(feh)(ghe) ; \quad . \quad (122)$$

subtracting, therefore, (122) from (121), we eliminate the symbol (fg), and obtain the type

II. $\quad (fee)(gee) - (fgg)(gff) = \Sigma'\{(fgh)(hff) - (feh)(ghe)\} ; (123)$

which represents in general a system of $n(n-1)(n-2)$ distinct and homogeneous equations of the second dimension, containing each $2(n-2)$ terms, and derived by eliminations of the kind last mentioned, from the formulæ (95), (97), (100), in a manner agreeable to the analysis of paragraph [14.]. Indeed, it was shown in that paragraph, that the equation

$$(gf) = (fg), \quad (100),$$

though known from earlier and simpler principles to be true, might be regarded as *included* in (95) and (97) ; but this need not prevent us from *using* that equation in combination with the others, whenever it may seem advantageous to do so : and other combinations of them may with its help be formed, which are occasionally convenient, or even sometimes necessary, although all the *general* results of the elimination of the symbols (fg) are sufficiently represented by the recent type II., or by the formula (123). For example, a subordinate type, including only $\frac{1}{2}n(n-1)(n-2)$ distinct equations, of $2(n-2)$ terms each, between the symbols (fgh), may thus be formed, by subtracting (95) from (97), under the condition that *efg* shall still denote some three unequal indices ; namely,

$$0 = \Sigma\{(feh)(ghe) - (geh)(fhe)\} ; \quad . \quad . \quad (124)$$

or more fully, but at the same time with the suppression of a few parentheses, which do not appear to be at this stage essential to clearness,

$$(fge)(eff + egg) = \Sigma'(geh . fhe - feh . ghe) : \quad . \quad (125)$$

this last formula admitting also of being obtained from (122),

by interchanging f and g, and subtracting. Again, a type which is in general still more subordinate, as including only $\frac{1}{2}n(n-1)$ distinct equations, of $2(n-2)$ terms each, may be derived by the same process from (120); namely the type,

$$0 = \Sigma(fgh)(hff + hgg); \quad . \quad . \quad . \quad . \quad (126)$$

or in a slightly more expanded form,

$$(fge)(eff + egg) = \Sigma'(gfh)(hff + hgg); \quad . \quad (127)$$

which may also be easily derived, in the same way, from (121). It will, however, be found, by pursuing a little further the analysis of [14.], that the equations of this last type, (126) or (127), are always consequences of the equations of the intermediate type, (124) or (125); the sum of the $n-2$ equations of the form (125), which answer to the various values of e that remain when f and g have been selected, being in fact equivalent to the formula (126). It will also be found, by the same kind of analysis, that the intermediate equations of the type (124) or (125) are *generally* deducible from those of the form (123). But on the subject of these *general reductions,* connected with the elimination of the symbols (fg) or (gf), it may be proper to add a few words.

[19.] Let us admit, at least as temporary abridgments, the notations

$$[fg] = \Sigma(fgh \cdot hff); \quad [fge] = \Sigma(fhe \cdot geh); \quad . \quad (128)$$

where e, f, g are any three unequal indices, and h varies under Σ, as before, from 1 to n. Then the formula (95) gives $n-1$ distinct equivalents for the symbol (fg), of which one is by (120) of the form $[fg]$, and the $n-2$ others are each of the form $[fge]$; in such a manner that we may write, instead of (95), with these last notations, the system of the two formulæ,

$$(fg) = [fg], \quad (fg) = [fge]; \quad . \quad . \quad . \quad (129)$$

whereof the latter is equivalent to a system of $n-2$ equations: and of course, instead of (97), we may in like manner write

$$(gf) = [gf], \quad (gf) = [gfe]. \quad . \quad . \quad . \quad (130)$$

The equations (99) may now be thus presented:

$$\left.\begin{array}{l} (n-1)(fg) = [fg] + \Sigma'[fge] = \Sigma\Sigma(fhe \cdot geh); \\ (n-1)(gf) = [gf] + \Sigma'[gfe] = \Sigma\Sigma(feh \cdot ghe); \end{array}\right\} \quad (131)$$

where e under the sign Σ' is distinct from each of the two indices f and g; but, under the double sign $\Sigma\Sigma$, both e and h may each receive any one of the values from 1 to n. The two double sums are equal, as in [14.], and therefore we must have, *identically,*

$$[fg] + \Sigma'[fge] = [gf] + \Sigma'[gfe]; \quad . \quad . \quad (132)$$

the equation (100) being at the same time seen again to be a

consequence, by simple additions, of the formulæ (95) and (97).
Thus, after assigning any two unequal values to the indices f
and g, we see that the two symbols, (fg), (gf); the two others,
$[fg]$, $[gf]$; the $n-2$ symbols, $[fge]$; and the $n-2$ symbols,
$[gfe]$, are indeed all equal to each other: but that the $2n-1$
equations between these $2n$ equal symbols are connected by a
relation, such that any $2n-2$ of them, which are distinct among
themselves, *include* the remaining one; and that therefore, after
the elimination of (fg) and (gf), there remain only $2(n-2)$
distinct equations of condition, as was otherwise shown in [14.].
But, in that paragraph, we proposed to form those resulting con-
ditions on a plan which may now be represented by the formulæ

$$[fg] = [fge], \quad [gf] = [gfe]; \quad . \quad . \quad . \quad (133)$$

whereas we now prefer, for the sake of the convenience gained
by the disappearance of certain terms in the subtractions, to
employ that other mode of combination, which conducted in
[18.] to the formula (123), and may now be denoted as follows:

$$[fg] = [gfe], \quad [gf] = [fge]. \quad . \quad . \quad (134)$$

Summing these last with respect to e, we find

$$(n-2)[fg] = \Sigma'[gfe], \quad (n-2)[gf] = \Sigma'[fge]; \quad . \quad (135)$$

and therefore, by the identity (132),

$$(n-3)[gf] = (n-3)[fg]. \quad . \quad . \quad . \quad (136)$$

If, then, $n > 3$, we are entitled to infer, from (123) or (134),
the following formula, which is equivalent to (126),

$$[gf] = [fg]; \quad . \quad . \quad . \quad . \quad (137)$$

and therefore also by (134) this other type, equivalent to (124),

$$[fge] = [gfe], \quad . \quad . \quad . \quad . \quad (138)$$

which includes $n-2$ equations, when f and g are given, and
conducts, reciprocally, by (132), to (137). *In general,* therefore,
if we adopt the type (134), we need not retain *also* either of these
two latter types, (137), (138). But in the particular *case* where
$n = 3$, that is, in the case of quadrinomes, the identity (132)
reduces the two equations (134) to one, after f and g have been
selected; and with this one we must then combine either of the
two equations (137) or (138), which in this case become iden-
tical with each other.

[20.] In particular, for this case of *qudrinomials* ($n = 3$), we
have with the notations (114) (128), the four following values
for (23), or for b_1 (compare again a note to [12.]):

$$\left.\begin{array}{l} [23] = 231 \cdot 122 + 233 \cdot 322 = l_1 n_1 - n_2 m_3 ; \\ [231] = 211 \cdot 311 + 231 \cdot 313 = -m_2 n_3 + l_1 m_1 ; \\ [32] = 321 \cdot 133 + 322 \cdot 233 = l_1 m_1 - m_3 n_2 ; \\ [321] = 311 \cdot 211 + 321 \cdot 212 = -n_3 m_2 + l_1 n_1 ; \end{array}\right\} \quad (139)$$

but, whether we equate the first to the fourth, or the second to the third of these expressions for b_1, in conformity with the type (134), we obtain only one common equation of condition, $n_2 m_3 = n_3 m_2$, equivalent indeed by cyclical permutation to three, namely to the following,

$$0 = n_2 m_3 - n_3 m_2 = n_3 m_1 - n_1 m_3 = n_1 m_2 - n_2 m_1 ; \quad . \quad (140)$$

which evidently agree with certain simple combinations of the six equations on the two last lines of (86). If however we compare either the first value (139) with the third, or the second of those values with the fourth, according to the type (137) or (138), we find by each comparison the common condition $l_1 n_1 = l_1 m_1$, and thus recover the three equations of the first line of (85). In this way then we may obtain the required number of six distinct equations, with two terms each, between the nine symbols (fgh), or $l_1 \ldots n_3$, for the case of quadrinomes, by elimination of the three symbols (fg), or of b_1, b_2, b_3.

[21.] For the case of *quines* ($n = 4$), the general theory requires that the corresponding elimination of the $6 = \frac{1}{2} n(n-1)$ symbols of this form (fg), or $b_1 \ldots c_3$, should conduct to $24 = n(n-1)(n-2)$ distinct equations of condition, with $4 = 2(n-2)$ terms each, between the $\frac{1}{2} n^2(n-1) = 24$ symbols of the form (fgh), or $l_1 \ldots u_3$, each equation thus obtained being homogeneous, and of the second dimension ; and that all these 24 conditions should be included in the formula (134), or in the single type (123). And in fact we thus obtain, by comparison of the six expressions for b_1, of which one is

$$b_1 = (23) = [23] = \Sigma(23h \cdot h22) = l_1 n_1 - n_2 m_3 - p_1 r_2, \quad (141)$$

the four following equations of condition, included in that type or formula :

$$\left.\begin{array}{l} 0 = [23] - [321] = [32] - [231] ; \\ 0 = [23] - [324] = [32] - [234] ; \end{array}\right\} \quad . \quad (142)$$

that is, with the notations $l_1 \ldots u_3$,

$$\left.\begin{array}{l} n_2 m_3 - m_2 n_3 = p_3 s_3 - p_1 r_2 = p_1 r_3 - p_2 t_2 ; \\ n_2 m_3 + u_2 u_3 = p_3 s_3 + l_1 m_1 = l_1 n_1 - p_2 t_2 ; \end{array}\right\} \quad . \quad (143)$$

while we have in like manner six expressions for c_1, of which one is

$$c_1 = [41] = \Sigma(41h \cdot h44) = -(r_1 u_1 + s_1 u_2 + t_1 u_3), \quad . \quad (144)$$

and of which the comparison conducts to the four other distinct conditions:

$$\left. \begin{array}{l} r_1u_1 - n_1r_2 = l_3t_3 + n_3t_1 = -l_2s_2 - t_1u_3\,; \\ r_1u_1 + m_1r_3 = l_3t_3 - s_1u_2 = -l_2s_2 - m_2s_1\,; \end{array} \right\} \quad . \quad (145)$$

where cyclical permutation of indices is still allowed. The equations obtained from the types (137) (138) would be found (as the theory requires) to be merely consequences of these; for example, by making $e=1$, $f=2$, $g=3$, $h=4$, those two types give only the conditions,

$$l_1(n_1 - m_1) = p_1(r_2 + r_3) = p_2t_2 + p_3s_3, \quad . \quad . \quad (146)$$

which are obviously included in (143).

[22.] With respect to those other homogeneous equations of the second dimension, between the symbols (fgh), which are obtained immediately, or without any elimination of the symbols (f), (fg), from the general conditions of association, and are included in the formula (96), they may now be developed as follows.

Making $k=f$ in (96), and then interchanging f and e, for the sake of comparison with (123), we obtain the type.

III. $\quad fgg \cdot gee - gff \cdot fee = \Sigma'(gfh \cdot hee)\,; \quad . \quad . \quad (147)$

which includes generally $\frac{1}{2}n(n-1)(n-2)$ distinct equations, of $n-1$ terms each. For quines, we have thus 12 equations of 3 terms sufficiently represented by the following:

$$\left. \begin{array}{l} n_1n_2 - m_2m_1 = p_3r_3, \quad n_1u_2 + m_2u_1 = -l_3u_3\,; \\ r_2m_2 + u_2r_1 = s_2r_3, \quad r_2n_2 - u_2r_3 = + t_2m_1\,; \end{array} \right\} \quad . \quad (148)$$

the value 4 being attributed to the index h or e, in forming the equations on the first line, but to f or g for the second line. For quadrinomes, the corresponding equations are only three, namely

$$0 = n_1n_2 - m_1m_2 = n_2n_3 - m_2m_3 = n_3n_1 - m_3m_1\,; \quad . \quad (149)$$

which however are sufficient, in conjunction with the three lately marked as (140), to reproduce the six equations of the two last lines of (86). In general, by adding and subtracting the two types (123) (147), we obtain the formula,

$$(fee \pm fgg)(gee \mp gff) = \Sigma'(fgh)(hff \mp hee) - \Sigma'(feh \cdot ghe)\,; \quad (150)$$

where, as a verification, if we take the lower signs, and interchange f and g, so as to recover the first member with the upper signs, the comparison of the two expressions for that member conducts to an equation between the two second members, which may also be obtained by the comparison of (125) and (127).

[23.] Again, making $f=e$ in (96), and then changing k to f, we obtain the formula,

IV. $\quad 0 = (egf)(eff + egg) + \Sigma'(egh \cdot ehf)\,; \quad . \quad (151)$

where *efgh* are again unequal indices. This IV.th type includes generally $n(n-1)(n-2)$ distinct equations, with $n-1$ terms each. For the case $n=3$ there arise thus 6 equations of 2 terms, namely the six on the two last lines of (85); so that the 18 equations (85), (86), for associative quadrinomials, have thus been completely reproduced, as consequences of the general theory. For the case of quines, the type (151) gives 24 equations of 3 terms, which may be represented as follows:

$$0=l_2(n_1-m_1)+p_2s_1=l_3(n_1-m_1)+p_3t_1\;; \left.\right\} \quad . \quad (152)$$
$$0=s_2(r_2+r_3)+\;t_1t_2=t_3(r_2+r_3)+s_3s_1\;;$$

$$0=t_1(u_1-m_1)+l_3s_1=p_2(u_1-m_1)+l_2p_3;\left.\right\} \quad . \quad (153)$$
$$0=s_1(u_1+n_1)-l_2t_1=p_3(u_1+n_1)-l_3p_2\;;$$

either h or e being $=4$ in (152), and either f or g having that value in (153), while 1, 2, 3 may still be cyclically permuted.

[24.] Finally, by supposing, as in (119), that *efgk* are four unequal indices, and that h under Σ'' is unequal to each of them, we obtain from (96) one other type, including *generally* $\frac{1}{2}n(n-1)$ $(n-2)(n-3)$ equations, of $n-1$ terms each, but furnishing no new conditions of association for quadrinomials: namely,

V. $efk \cdot gee + fgk \cdot egg + egk \cdot fkk = \Sigma''(fhk \cdot geh).$ (154)

For quines, the sum Σ'' vanishes, and we obtain twelve equations of three terms each, which may (with the help of permutations) be all represented by the four following:

$$0=l_1r_2-u_2s_3-n_3t_2=l_1r_3-m_2s_3+u_3t_2,\left.\right\} \quad . \quad (155)$$
$$0=l_1r_1+n_2s_3+m_3t_2=n_1p_1-m_2p_2-u_3p_3\;;$$

where the index 4 has been made to coincide with e or with g in the first line, but with f or k in the second.

[25.] In general, the number of distinct associative equations, included in the three last types (147) (151) (154), or III. IV. V., which have been all derived from the formula (96), and have been obtained without elimination of (f) or (fg), amounts in the aggregate to

$$\tfrac{1}{2}n(n-1)(n-2)+n(n-1)(n-2)+\tfrac{1}{2}n(n-1)(n-2)(n-3)$$
$$=\tfrac{1}{2}n^2(n-1)(n-2)\;;\quad . \quad . \quad . \quad . \quad . \quad . \quad (156)$$

as, by the analysis of [14.], it ought to do. And when we add this number to the $n(n-2)$ of the type I., or (113), and to the $n(n-1)(n-2)$ of the type II., or (123), obtained by such elimination, we have in all this other number,

$$\tfrac{1}{2}n^2(n-1)(n-2)+n^2(n-2)=\tfrac{1}{2}n^2(n+1)(n-2),\quad . \quad (157)$$

of distinct and homogeneous equations of the second dimension, between the $\frac{1}{2}n^2(n-1)$ symbols of the form (fgh): as, by the

formulæ (109) (110) of [15.], we ought to have. As regards the *signification* of the five foregoing principal types, which it has been thought convenient to distinguish among themselves, and to arrange according to the various ways in which they involve the symbols of the form (*eff*), it will be found, on reviewing the analysis employed, that they all express *ultimately* only consequences of that *one* very simple and useful formula,

$$V\iota V\iota'\iota'' = \iota''S\iota\iota' - \iota S\iota''\iota, \quad (67)$$

which, with a slightly different notation, has been elsewhere shown to be so important in the Calculus of Quaternions[*]. In fact, the equations (95)...(98), on which those five separate types have been founded, may all be deduced from (67) and (69), whereof the latter is a consequence of the former.

Observatory of Trinity College, Dublin,
August 14, 1854.

[To be continued.]

XXXV. *On the Elastic Forces of Vapours* in vacuo *and in Gases, at different Temperatures; and on the Tensions of the Vapours furnished by mixed or superposed Liquids.* By M.V. REGNAULT[†].

ACCORDING to the views which I have adopted regarding the mode of generation of the power in machines moved by elastic fluids, the motive power produced by the expansion of any elastic fluid is always in proportion to the loss of heat undergone by this fluid in the part of the machine where the power is produced.

During the last few years, several distinguished geometricians have endeavoured to deduce this principle from abstract considerations, founded upon hypotheses of greater or less probability. For my own part, I have long laboured to bring together the experimental data, by means of which the theoretical motive power produced by a given elastic fluid which undergoes a certain change of volume, as well as the quantity of heat which becomes latent in consequence of this change, might be calculated *à priori*. Unfortunately these data are very numerous, and most of them can only be determined by extremely delicate and difficult experiments.

One immediate consequence of the principle which I have just enunciated is the following :—

When elastic fluids of different natures produce an equal amount of power, they must lose equal quantities of heat.

In order to submit this law, deduced from the general prin-

* See the author's Lectures on this subject, cited in former notes to this paper.
† From the *Comptes Rendus* for August 14, 1854.

ciple, to experimental verification, we must know,—I. the total amount of heat contained in different elastic fluids at a certain state of temperature and pressure; II. the relations which exist between the temperature and pressure for the same volume of these different fluids. The knowledge of these elements is particularly important in the case of readily condensable vapours, which are still the only elastic fluids employed in engines.

In my preceding memoirs, which compose the twenty-first volume of the *Mémoires de l'Académie des Sciences*, I have published the results of my experiments on the elastic force of aqueous vapour at saturation at different temperatures, and on the total quantities of heat which this vapour at saturation possesses under different pressures *. In memoirs since presented to the Academy, which will shortly be published *in extenso* in its *Mémoires*, I have given the capacity for heat under a constant pressure of a great number of permanent gases and super-heated vapours, as well as the calorific changes undergone by permanent elastic fluids during their expansion effected under certain conditions.

I now propose to bring before the Academy some experiments which I have made on the elastic force, at different temperatures, of saturated vapours other than that of water. I shall add the results of numerous experiments which I have made in studying the phænomenon of vaporization *in vacuo* and in gases; a phænomenon on which science as yet possesses but very vague notions, deduced from a small number of experiments which are in themselves very inconclusive.

Although the greater part of these experiments were executed between 1843 and 1850, I had proposed to defer their publication still longer, in the hope that I might be able to complete them. But many physicists being now occupied upon the same subject, I am compelled to give at all events a brief statement of the results at which I have now arrived. I shall divide this note into five parts.

The first will include the results which I have obtained regarding the elastic forces of vapours at saturation furnished by a certain number of liquids, selected amongst those most easily obtained in a state of purity, in large quantity and at a price which does not preclude their employment in machinery.

In the second part I shall treat of the elastic forces of saline solutions, and of the application which may be made of them in the study of various questions in physics and molecular chemistry.

The third will include the phænomena of the vaporization of liquids in gases.

The fourth will contain the results of my experiments on the

* [See Taylor's Scientific Memoirs, vol. iv. p. 559.]

elastic force of the vapours furnished *in vacuo* by volatile liquids, mixed or superposed.

Lastly, in the fifth I shall give the results of the experiments which I have made to decide whether the tension acquired *in vacuo* is, or is not, dependent upon the solid or liquid form of the body which furnishes it.

Part I.—*On the Elastic Forces of Vapours at Saturation* in vacuo.

I shall not stop to describe the processes which I have employed in determining the elastic forces of saturated vapours *in vacuo*. These processes are similar to those applied by me to the vapour of water, which are described in detail in vol. xxi. of the *Mémoires de l'Académie.*

Nor do the limits assigned to this note allow of my giving the immediate results of my determinations, which are very numerous. I shall merely give the elastic forces at every 10° C. of the following liquids, which I have studied between the most extended limits. These are alcohol, æther, sulphuret of carbon, chloroform, and essence of turpentine.

Tensions of the vapour of alcohol.		Tensions of the vapour of æther.		Sulphuret of carbon.		Chloroform by tension *in vacuo.*		Essence of turpentine.	
°	mm	°	mm	°	mm	°	mm	°	mm
− 21	3·12	− 20	69·2	− 16	58·8	+ 10	130·4	0	2·1
− 20	3·34	− 10	113·2	− 10	79·0	20	190·2	10	2·3
− 10	6·50	0	182·3	0	127·3	30	276·1	20	4·3
0	12·73	10	286·5	10	199·3	36	342·2	30	7·0
10	24·08	20	434·8	20	298·2			40	11·2
20	44·0	30	637·0	30	434·6	By the method		50	17·2
30	78·4	40	913·6	40	617·5	of ebullition.		60	26·9
40	134·10	50	1268·0	50	852·7			70	41·9
50	220·3	60	1730·3	60	1162·6	...	...	80	61·2
60	350·0	70	2309·5	70	1549·0	36	313·4	90	91·0
70	539·2	80	2947·2	80	2030·5	40	364·0	100	134·9
80	812·8	90	3899·0	90	2623·1	50	524·3	110	187·3
90	1190·4	100	4920·4	100	3321·3	60	738·0	120	257·0
100	1685·0	110	6249·0	110	4136·3	70	976·2	130	347·0
110	2351·8	116	7076·2	120	5121·6	80	1367·8	140	462·3
120	3207·8	...	......	130	6260·6	90	1811·5	150	604·5
130	4331·2	...	......	136	7029·2	100	2354·6	160	777·2
140	5637·7	...	......	...	......	110	3020·4	170	989·0
150	7257·8	...	......	...	......	120	3818·0	180	1225·0
152	7617·3	...	......	...	......	130	4721·0	190	1514·7
...	......	...	......	...	......	...	...	200	1865·6
...	......	...	......	...	......	...	...	210	2251·2
...	......	...	......	...	......	...	...	220	2690·3
...	......	...	......	...	......	...	...	222	2778·5

These results have been obtained, either by the determination of the elastic forces *in vacuo,* or by determining the temperature of the vapour of a boiling liquid under the pressure of an

artificial atmosphere. The former method has been followed for the low temperatures; the second has been exclusively employed in the high temperatures. In all cases they are arranged in such a manner that the curves of the elastic forces given by the two methods should present a common part by which we may judge of their coincidence. I have already shown, in my memoir on the elastic force of water vapour, that this coincidence is exact in the case of water, the two methods giving perfectly identical results. I have ascertained that the same is the case with other volatile liquids, provided they are in a state of perfect purity. When a liquid contains even an extremely small portion of another volatile substance, the two methods give different values for the elastic force of its vapour at the same temperature; so that this furnishes an extremely delicate means of judging of the homogeneity of a volatile body.

Sulphuret of carbon is easily obtained in a state of purity, but the same cannot be said of alcohol and æther. As to chloroform, whatever care may be taken in its preparation, it always contains several intermixed substances, which it is impossible to separate by fractional distillation even when operating upon large masses. The vapour has a different elastic force and a sensibly different density, according as the first or the last products of distillation are operated with. Chloroform has also constantly given different values for its elastic force at the same temperature, according to the method by which it was determined. This circumstance is easily seen from the preceding table, in which I have only given a single series of the experiments which I have made upon chloroform.

Some liquids become modified in their molecular constitution when boiled for a long time under high pressures. It thus often happens that at the end of a series of experiments the liquid is found not to have the same boiling-point under the ordinary pressure of the atmosphere as at the commencement. Essence of turpentine offers a remarkable example of this. Thus, a considerable quantity of the essence (30 or 40 litres) having been boiled for several hours under a pressure of 7 to 8 atmospheres, became almost completely converted into a liquid which boiled at a temperature above 230° C. under the ordinary atmospheric pressure. I put this modified liquid aside in order to determine its nature, but it has since been thrown away by mistake.

Other liquids appeared to undergo molecular modifications, rendered sensible by the tension of their vapours, when left by themselves for a long time in hermetically closed tubes. Æther has presented me with a curious example of this. I shall return to this subject on another occasion.

I may remark, in conclusion, that the method of ebullition in

artificial atmospheres, the thermometers being immersed in the vapour, necessarily gives accurate results for homogeneous liquids when the actual pressure is exactly determined; for it is by this method that the boiling-point of thermometers is fixed. But when solutions of fixed substances in volatile fluids, or mixtures of several unequally volatile matters are in question, the tension of the vapour may be very different according as it is determined in the *static condition,* if I may use the expression,—the vapour and the volatile liquid being both immersed in a medium of invariable temperature,—or in a *dynamic state,* under the influence of a current of heat which traverses the apparatus, the liquid receiving the heat which produces the vaporization, whilst the vapour is subjected to refrigerating agencies which produce its partial condensation. The limits which I am compelled to observe in this note prevent me from giving further development to these considerations.

Part II.—*On the Boiling-points of Saline solutions.*

Everyone knows that saline solutions require a higher temperature to boil them than pure water under the same pressure. For a given salt the excess of temperature increases in proportion to the quantity of matter dissolved. All soluble substances do not possess in the same degree the faculty of retarding the boiling-point of the water in which equal weights of them are dissolved. This faculty does not depend only upon their solubility; it appears to result principally from a special affinity of the substance for water.

Rudberg has made the very curious observation, that when concentrated saline solutions are kept boiling at temperatures much above 100° C. under the ordinary pressure of the atmosphere, the vapours which they emit nevertheless only possess the temperature which they would present if they were disengaged from pure water in ebullition under the same pressure. Rudberg has made a great number of experiments on the most various solutions, and with very exact instruments. The conclusion which he has drawn from them is as follows :—*Whatever temperature a solution must attain before boiling, the vapour never presents a higher temperature than that which it would have had if it had been disengaged from pure water ; in other words, it presents the temperature at which the tension of this vapour, at saturation* in vacuo, *is in equilibrium with the pressure under which the ebullition takes place.* This conclusion must of course refer, not only to the saline solutions experimented upon by Rudberg, but also to all solutions in a volatile liquid of substances which are fixed at the temperature of its ebullition.

It is not difficult to account for this circumstance, that a saline

solution must boil at higher temperatures than the volatile liquid alone. Thus it may be conceived, that when the pure volatile liquid is submitted to the action of heat, its molecules, in order to assume the state of vapour, have only to overcome the external pressure to which they are subjected, and the adhesion or special affinity which they possess for the similar molecules which retain the liquid form. In the case of a saline solution, on the other hand, the molecules which assume the form of vapour have also to overcome the attraction exerted upon them by the particles of the dissolved substance, an attraction which is generally greater than that arising from similar particles. It is consequently necessary for the development of vapour that the fluid medium should assume a higher temperature than if it consisted solely of the volatile substance.

But I do not so clearly perceive how the vapour, at the moment of its disengagement from the liquid, should possess a much lower temperature than that of the last strata of liquid which it has just traversed. I admit that the vapour, at the moment of its formation in the midst of the solution, possesses an elastic force greater than that which gives an equilibrium to the external pressure, since it must also overcome the attractive force of the saline particles. But as soon as this vapour has collected in a bubble rising through the liquid, it must expand, and only preserve the elastic force which is necessary to place it in equilibrium with the hydrostatic pressure which it experiences in the stratum of liquid in which it is at the moment, and with the capillary action of the liquid walls of the bubble, an action which diminishes in proportion as the bubble acquires greater development. I admit that, in consequence of this successive expansion, the temperature of the vapour must be lowered; but as the bubble is surrounded by a hotter liquid, this ought constantly to furnish the heat which is lost by expansion; and the bubble, on issuing from the liquid, ought to present an equilibrium of temperature with this.

To explain Rudberg's law, it is necessary to assume that the vapour, as long as it remains in the midst of the boiling liquid, possesses, in consequence of the attraction of the saline particles, a greater density than corresponds under the same temperature with the hydrostatic pressure exerted upon it; and that it only assumes its normal density at the moment when, escaping from the liquid, it is relieved from this action. The vapour would then experience a sudden dilatation, which would cause the excess of heat to become latent, and bring it *exactly* to the temperature at which its elastic force is in equilibrium with the atmospheric pressure.

But, on the one hand, it is necessary to admit that this excess

of density continues, whatever volume the bubble may acquire in rising through the liquid; for I have ascertained that the temperature of the vapour is the same, whether the solution boil quickly or slowly, and that it is also the same when the liquid rises to a great height above the heated bottom, although in this latter case the bubbles often acquire a very considerable volume before bursting at the surface of the liquid.

On the other hand, in order to explain the great diminution of temperature undergone by the vapour at the moment of its escape from a boiling solution strongly impregnated with certain salts, admitting the results which I have obtained as to the quantity of heat which becomes latent by the expansion of elastic fluids, we must suppose that the bubble of vapour whilst still in the midst of the fluid, experiences a very considerable excess of compression, much greater, indeed, than can reasonably be admitted.

I have, however, instituted some experiments with a view to ascertain whether the fact proved by Rudberg arises from a general law, such as that enunciated by him, or whether it must be attributed simply to the circumstances under which his experiments were made.

I first of all wished to ascertain whether this phænomenon is presented with the same constancy when saline solutions are boiled under pressures very different from the ordinary atmospheric pressure, for all Rudberg's experiments were made under the latter. I made use of the small copper boiler in which I made my former determinations of the elastic force of the vapour of water (*Mém. de l'Acad.* vol. xxi. p. 515). The lid of this boiler is traversed by four tubes hermetically closed at their lower extremities; two of these tubes descend into the boiling fluid, and the other two reach no further than the vapour. These tubes contain a small quantity of mercury, in which the reservoirs of the thermometers are immersed, so that these are thus protected from the internal pressure. The disengaging tube of the boiler communicates with a refrigerator, which serves to condense the vapour; and this refrigerator itself communicates with a large reservoir of air, of which the pressure may be varied at will.

Into the boiler I put concentrated solutions of chloride of calcium, which I caused to boil under pressures sometimes weaker, sometimes stronger than the ordinary pressure of the atmosphere, and noted the temperatures indicated simultaneously by the thermometers immersed in the vapour and those which descended into the liquid. The results obtained are given in the two following tables, of which the second refers to a solution containing a larger quantity of salt.

Table I.			
Pressures under which the temperatures are attained.	Temperature of the liquid.	Temperature of the vapour.	Temperature of the vapour of distilled water under the same pressure.
mm 82·52	52·0	47·88	47·84
136·61	61·58	58·20	58·16
219·44	71·80	68·73	68·61
286·43	......	74·94	74·84
434·19	87·54	85 09	85·07
757·22	......	99·88	99·90
1807·15	129·86	126·63	126·16
2182·35	136·30	132·92	132·42
2702·13	142·79	140·35	139·81
3123·69	147·91	145·57	145·00
Table II.			
57·83	......	41·15	41·00
58·45	......	41·25	41·17
59·09	......	41·41	41·37
133·07	......	57·78	57 63
198·25	......	66·46	66·31
198·41	78·45	66·50	66·35
282·92	79·1	74·65	74·17
283·68	......	74·72	74·59
362·49	85·1	80·65	80·56
479·17	91·1	87·68	87·59
754·71	102·2	100·00	99·81

The inspection of these tables shows that the thermometer immersed in the vapour constantly indicates a temperature a little higher than that of the vapour of pure water under the same pressure; but the difference is small, and may perhaps be attributed to the radiation of the hot liquid and the drops of fluid which are thrown up in abundance by saline solutions during ebullition. As to the thermometers which descend into the boiling liquid, their course is extremely irregular, presenting rapid variations which often rise to several degrees. It is impossible to deduce anything certain from the indications which they furnish.

It may consequently be admitted that the phænomenon observed by Rudberg with regard to saline solutions in ebullition under the ordinary pressure of the atmosphere, is also presented when they are boiled under pressures greater or less than this.

To observe more readily the circumstances under which the phænomenon occurs, I made some experiments in a glass balloon with a wide mouth, upon mixtures of water and sulphuric acid in

variable proportions, taking care never to add sufficient sulphuric
acid to allow any portion of that substance to pass by distillation.
Into the neck of the balloon I fitted two wide copper tubes,
placed one within the other like the tube of a telescope. The
upper tube carried towards its summit two lateral tubulures,
affording an exit for the vapour; its upper orifice was closed by
a cork, through which the stem of a very sensitive thermometer
was passed. By this arrangement it was easy to place the bulb
of the thermometer in any region of the balloon, still keeping
the entire mercurial column in the vapour.

Operating in this manner, it is soon seen to be impossible
to find any position in the balloon, where the bulb of the ther-
mometer does not constantly get covered with water, which
falls back, drop by drop, into the boiling liquid. Now every one
will understand that if the instrument be constantly moistened
with condensed vapour, it can never indicate a higher tempera-
ture than that at which the pure liquid boils under the same
pressure. It is therefore evident that no experiment in which
the thermometer becomes wet can prove anything in favour of
Rudberg's law; and there can be no doubt that this has taken
place in the experiments of that physicist.

The greater part of the water which runs down the thermome-
ter is produced by condensation on the upper part of the stem.
In order to prevent this water from reaching the reservoir, I
attached to the stem immediately above the reservoir a very thin
metallic disc, which collected it; a second similar disc, attached
to the former by means of thin wires hung below the reservoir,
so as to protect this at once from the radiation of the super-
heated liquid and from the drops of the solution which are
always projected in abundance by boiling liquids. The bulb of
the thermometer was not more than 8 millimeters in diameter.

Even with this arrangement it is very difficult to place the
thermometer in such a manner as that the bulb shall not be-
come wetted. As long as the bulb is at a distance of more than
3 or 4 centimeters from the boiling solution it always becomes
wet, and then it can never indicate any other temperature than
that of the ebullition of pure water. But when it is lowered so
as to go nearer the liquid, the temperature rises, the reservoir at
the same time becoming dry. The temperature thus continues
to rise until the bulb touches the liquid.

The region of the balloon in which the thermometer indicates
a temperature above that of pure boiling water, may usually be
recognized even by sight; it is that in which the inner walls of
the balloon remain dry, whilst the upper portions of its walls
are always moistened by condensed drops. The height of the
stratum of super-heated vapour depends upon the temperature

of the boiling liquid, and especially upon the briskness of the ebullition.

The observations which I have just described confirm the fact announced by Rudberg, but it appears to me that they also show its cause; for whenever the thermometer only indicates the temperature under which pure watery vapour is in equilibrium with the external pressure, we find that the reservoir is wet. On the other hand, the instrument shows a higher temperature when its reservoir is dry, a condition which I have only been able to realize in the strata of vapour immediately above the super-heated liquid.

I think, therefore, that the vapour, originating in boiling saline solutions, is equal in temperature to them, and does not possess an elastic force much superior to the hydrostatic pressure which they exert upon it. If the temperature of this vapour be suddenly lowered to the degree which corresponds with its saturation under this pressure, this arises from the circumstance that in consequence of the small capacity for heat possessed by vapours in comparison to their volume, the excess of heat is rapidly absorbed by external refrigerating causes, and especially by the vaporization which acts upon the infinity of minute liquid globules, which are constantly projected into the atmosphere of vapour, at the moment when the bubbles burst at the surface of the boiling liquid.

I have determined, with several aqueous solutions, the temperature to which they must be raised in a manometric apparatus, in order that the vapour, thus produced *in vacuo*, should be in equilibrium with a pressure of 760 millimeters. The excess of this temperature over that of 100° C. which would give water vapour this tension of 760 millimeters, if it were in presence of pure water, may seem, as M. Plücker has lately pointed out, to measure the excess of affinity possessed by watery vapour for the saline substance, in comparison with that which it presents for the similar particles of water. But in order that this affinity, thus measured, may constitute a specific character of the substances, it is necessary that it should vary in proportion to the quantity of the same salt in the solution. Now I have found that this is not the case; the variation follows a more complex law, which appears to depend upon the nature of the salt.

I attach particular interest to the comparison of the temperature at which the vapour given off *in vacuo* by a saline solution is under a pressure of 760 millimeters in equilibrium, with the temperature presented by the same solution when boiling under the same pressure. Unfortunately, it is nearly impossible to determine, with any degree of precision, the boiling-point of a concentrated saline solution. The ebullition is always irregular;

it proceeds by jerks and starts, and the thermometer indicates sudden variations, which sometimes exceed 10° C. We know besides that the temperature of ebullition varies according to the nature of the vessel and the form of its walls.

The ebullition of a liquid is a very complicated phænomenon, especially when the liquid is not homogeneous. Molecular forces, of which the nature is still but little known, play an important part in it. It is impossible to get rid of these complex actions, and only take into consideration, in the study of this phænomenon, the pressure of the external atmosphere and the temperature of the boiling liquid.

But if it be nearly impossible to deduce any certain results from the determination of the temperature of ebullition of saline solutions, it is very different with the elastic forces of the vapours given off by these solutions *in vacuo.* These may be determined with great precision, and I have no doubt that the study of them will hereafter furnish a most valuable means of ascertaining the chemical phænomena which take place in solutions. I have convinced myself, in fact, that as soon as a phænomenon of this nature takes place with dissolved substances, it betrays itself by a singular point in the curve of the elastic force of the vapour furnished by the solution.

I shall give a few examples of the phænomena which may be studied by this method.

It is well known that certain salts crystallize from their aqueous solutions with different amounts of water, according to the temperature at which crystallization takes place. We may inquire whether this water combines with the salt in the midst of the liquid, or whether the combination only takes place at the moment of crystallization. The sulphates of soda, iron, copper, manganese, &c. present very curious examples of these modifications.

It will be interesting to compare the variations undergone by the elastic force of the vapour furnished by a saline solution at different temperatures, with the variations in the solubility of the salt under the same circumstances.

Lastly, when we shall have ascertained the law by which we may calculate the elastic force of the vapour furnished by the mixture, in known proportions of two solutions which exert no chemical action upon one another, from the elastic forces of the vapours given off by the separate solutions, we shall be able to ascertain whether double decompositions take place in the solutions themselves, or only at the moment of precipitation.

These examples, which I might readily multiply, will suffice to show that the study of the elastic forces of the vapours given off by solutions, will furnish a most valuable mode of in-

vestigation in the study of a multitude of phænomena in molecular chemistry; the results to be anticipated from which may be as important as those which M. Biot has deduced from the study of rotatory polarization. This method, besides, will possess the advantage of a more extended application.

In regard to these subjects, I have only been able to make a series of observations upon the sulphates mentioned above. Notwithstanding the lively interest which I attach to researches of this nature, I have been compelled to abandon them for the present, as they would have led me too far from the principal object towards which my efforts must be directed.

[To be continued.]

XXXVI. *Note on a Graphical Method of clearing a Lunar Distance. By* Sir J. W. Lubbock, *Bart., F.R.S. &c.**

BY the principles of the Gnomonic projection, to delineate an arc of any given number of degrees beginning from any given point S and in any given direction SR, the following construction obtains.

Let CPV be drawn perpendicular to the line PR through C the centre of the picture, and make VP in CP produced $= \sqrt{D^2 + CP^2}$, D = CB being the distance of the picture. D is a constant quantity, and is to be taken of any convenient length. At V make RVS equal to the given angle. RS is the required delineation.

In order to find graphically the angles A B C of a spherical triangle, of which the sides a, b, c are given, we have—
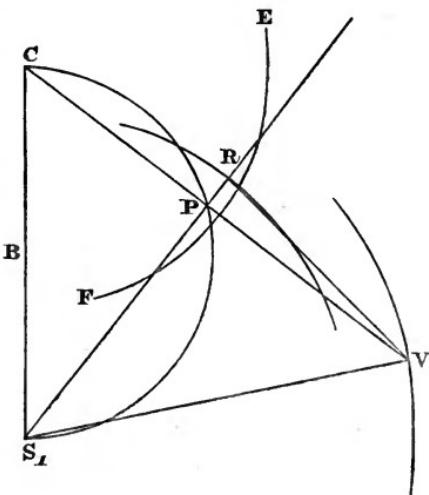
1st. To delineate the triangle correctly by the rules of perspective.

2nd. To ascertain the values of the original angles by means of their delineations in the picture.

Suppose, for example, we have the apparent zenith distances or altitudes of two stars S_1, S_2, and their apparent angular distance; if we place Z the zenith at C the centre of the picture, the following construction obtains for the accurate delineation of the spherical triangle $S_1 C S_2$:—

Take CS_1 representing the apparent zenith distance ζ_1 of S_1,

... CS_2 ... ... ζ_2 S_2,

and with radius CS_2 describe a circular arc EF. CS_1 and CS_2 are the natural tangents of ζ_1, and ζ_2 to radius D.

From S_1 draw any line S_1P, and lay down upon it S_1R representing the angular distance $S_1 S_2$ by the first rule. In this way one point is obtained of the conic section which represents the small circle described about S_2, whose intersection with the circle EF described with radius ZS_2 determines the place of S_2. In the same manner any number of points in the conic section may be found; but as it is easy to see very nearly where S_2 is situated, it will generally be sufficient to find two points on the conic section in the presumed vicinity of S_2, and the intersection of a straight line joining them with the circle will give the place of S_2 very nearly. The locus of the point P is a circle described with radius $\frac{1}{2}S_1C$, and the locus of the point V is a circle described with radius S_1V, $S_1V^2 = S_1C^2 + D^2$. If R is near the true place of S_2, S_2 will be found nearly by the intersection of a circular arc described with radius S_1R and the circular arc EF. S_1CS_2 is the accurate delineation in perspective of the spherical triangle S_1ZS_2.
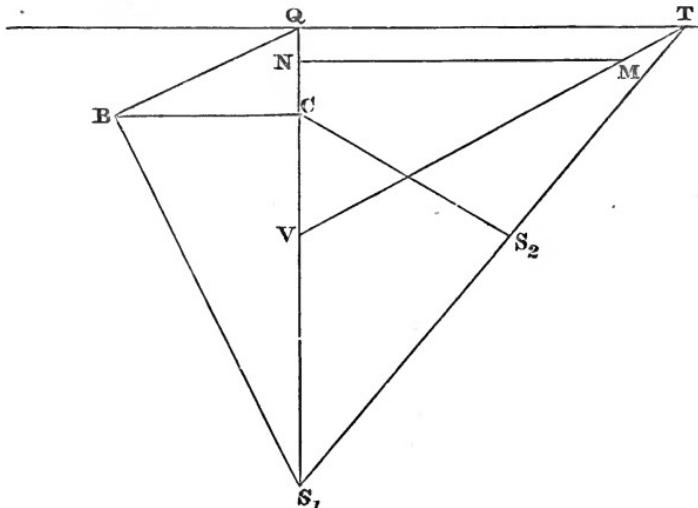
To measure the angle ZS_1S_2, produce S_1C to Q, making $S_1Q = 90°$, by making S_1BQ a right angle, and CB perpendicular to $CQ = D$.

Draw QT perpendicular to S_1Q, and in S_1Q take

$$QV = \sqrt{D^2 + CQ^2} = BQ.$$

Produce S_1S_2 cutting QT in the point T and join VT, the original angle ZS_1S_2 is equal to QVT.

As the correction in the distance for S_1 is approximately equal to the correction in altitude of S_1 multiplied by the cosine of the angle QVT, on VT take from scale the correction of S_1 in altitude $= VM$, draw NM parallel to QT, then VN is the correction in distance on the same scale approximately.

In the same way the angle ZS_2S_1 may be measured, and the correction of the distance due to the error of altitude of S_2 may be found.

When the point Z is placed at C the centre of the picture, the angle on the picture S_1ZS_2 is equal to the original angle S_1ZS_2.

The point Z may be placed anywhere in the picture, and the proper construction may be inferred from what precedes; but then the circle described with radius ZS_2 will be replaced by a conic section, and the angle S_1ZS_2 will not be equal to the original angle S_1ZS_2.

XXXVII.—I. *An Attempt to show the Mode of Origin of the Cell-Membrane, and the Nature of the Earliest Pores.*

II. *Remarks on the Article " Ovum " in the Cyclopædia of Physiology.*
By Martin Barry, *M.D., F.R.S.*[*]
[With a Plate.]

THE discoveries of Keber on the porosity of bodies have reminded me of some drawings of mine from nature, which lie buried in two rows of figures at the foot of a Plate in the Phil. Trans. for 1841 [†];—drawings which, from some remarks I made at the same time, it will be seen that I then believed to illustrate the mode of origin of the membrane of the cell [‡]. Further, there being among those drawings one which has long seemed to me to present the earliest pores, I will reproduce a few of them on this occasion.

There is first seen a large mother-cell. The nucleus of this divides into many parts—each part being itself a nucleus. From their origin in a mother-cell, these nuclei, after their liberation, are found in groups. They form cells, which are elliptical at first, become more spherical, and are often seen to be tapered at one end. (What the cells in question are, will be stated further on.)

Plate II.[*] fig. A represents one of the nuclei now mentioned as arising from division of the nucleus of a mother-cell. It has a nucleolus. At B such a nucleus has become segmented, and that

which in A was a nucleolus has in B passed into the state of nucleus. The segments in B are nucleolated nuclei. Fig. C represents a stage somewhat more advanced; the nucleus is vesicular (its nucleolus parietal). In D such segments as those at B and C have divided into groups of nucleolated nuclei, *g*. In fig. E some of these groups remain; others have broken up into single nucleolated nuclei. The latter are arranged so as *to lay the foundation of a membrane, h. This membrane is the primary membrane of a cell.* (Its elements usually appear first as mere globules, in which no nucleolus can be discerned, and which are too minute for examination.) At a later period I noticed the state fig. F. In F were nuclei such as those at *h* in E, and the nucleoli were most regularly equidistant. Up to this period the formation of the membrane is incomplete, and it continues incomplete until fibre forms. The nucleolated nuclei *h* of E and F are the elements of fibre.

From these drawings it will be seen that, according to my observations, the cell-membrane does not form *around* the nucleus, but is the outer part of the nucleus in an altered state. The nucleus, in fact, *passes into* the condition of a cell. In order to this it becomes segmented. The segments are usually globules or granules, too minute for examination. These coalesce and thus lay the foundation of a membrane. But there is one nucleus—that in fig. A—the segments of which are of sufficient size to show distinctly that they are nuclei themselves, each of these having its nucleolus, fig. B. The great size of the segments of this nucleus, no doubt, has reference to the functions which the cell it passes into is destined to perform, as well as to the enormous magnitude which it attains. That cell is my *ovisac*. It remains to add, that, having discerned traces of a like process in the formation even of the most transitory cells, I believe that *the membrane of every cell has the same mode of origin as the membrane of the ovisac.*

When at Giessen in 1852 I had the pleasure of reading the work of Baron Liebig, entitled " Untersuchungen über einige Ursachen der Säftebewegung im thierischen Organismus,'1848," and pointed out to that justly celebrated man the nucleoli in my drawing now reproduced in fig. F, stating to him my belief that it represented pores. I am still of the opinion that in the half-formed state of membrane fig. F, *nucleoli are the only pores.* And in proof that such is the case, have only to refer to what I have been maintaining since the year 1840, when, in the Phil. Trans. for that year I showed a suctorial power to be manifested by the protrusion of the parietal uncovered hyaline nucleolus of the germ spot, to take up the equivalent, corresponding, or counterpart from the spermatozoon ; and to drawings I gave in

the following year representing cells, the membranes of which, like that of the germ vesicle, presented in the situation of the nucleolus an *orifice**.   I lately wrote, " In mere nuclei, too, it is often seen that at the very part where the nucleolus lies the nucleal investment ceases †."   My belief, indeed, is, that at some period every nucleus has in its nucleolus a pore.

I am anxious to point out a connexion between nucleoli and pores, because of what I have for years been endeavouring to prove,—that in absorption, assimilation, reproduction and secretion, the nucleolus is the prime mover.

But other pores exist, as pointed out by Keber—spaces between the filaments and scales, far more capacious than nucleoli ; which larger pores I do not suppose to represent more than passages.

II. *Remarks on the Article " Ovum " in the Cyclopædia of Physiology.*

I am glad to see from what has just been published of this luminous and comprehensive article, that the observations of its excellent author enable him to confirm my own, published sixteen years before (in the Phil. Trans. for 1838), that the germ vesicle is the part which first arrives at full formation, and that my ovisac is formed around it‡.   Still more gratifying would it have been to find that Prof. Allen Thomson was aware of some observations I published three years afterwards—Phil. Trans. 1841 ; for, besides establishing the two facts just mentioned, as published in 1838, my later ones enabled me to record the first with far more minuteness, as well as to make known several others.   Of the drawings I gave in 1841, the Professor was evidently not aware, or he would have seen that they represent Nature even more accurately than his own.   They not only show the order of formation of the parts in question, but also that what he terms granules are *nucleolated nuclei*. They further show that these nuclei lay the foundation of the membrane of the ovisac.   I have only to add, that, buried as my said drawings of 1841 have been with a host of others, in a memoir, the title of which did not denote connexion with the ovum, I am not surprised at the oversight.   I have exhumed a few of the said drawings; and *it is these that I reproduce in Plate* II. (*figs.* A *to* F).

Once more referring to these drawings, I have now more fully to explain them.   In figs. C to E, *c* is the germ vesicle with its

* Phil. Trans. 1841.
† Monthly Journal of Medical Science, July, 1854.
‡ The ovisac acquires a vascular covering, and there is thus formed a " Graafian follicle."   M. Barry, Phil. Trans. 1838.

originally single spot. (Thus the germ vesicle is formed out of what in A was a nucleolus, and in B had become a nucleus.) In figs. E and F, *h* is the ovisac, or rather the foundation of its membrane. In fig. E, *g* represents the granular contents of a future cell—the ovisac. They are part of the nucleus, fig. A. Out of these granular contents (*g*, fig. E) are formed the albumenous contents of the ovisac, including von Baer's membrana granulosa and my retinacula, as well as the yelk and vitelline membrane*.

From my observations then it appears that the ovisac and all within it are formed out of a nucleus—fig. A ; that the ovisac is the *cell*, and that the germ vesicle is what was once a nucleolus. This nucleolus (young germ vesicle) often divides into two or more parts, and then the ovisac comes to include a plurality of ova; for a yelk forms around each part of the divided nucleolus.

Besides what has now been mentioned, the said article "Ovum" in the Cyclopædia of Physiology adds yet another proof to many in my experience, that it is often unfortunate for an observer to have the exploring of *new ground.* His observations may remain so long disregarded or denied, that when at length, after the lapse of no small fraction of a century, they come to be confirmed, he has the pain of exhuming them, all remembrance thereof being gone. I have had large experience of this, and am now compelled to endure the pain of more.

Thus Prof. Allen Thomson was certainly not aware of what I had recorded in 1839, or he would not have considered altogether "novel" some views published in Germany in 1852.

H. Meckel, 1852—as reported in the article " Ovum," 1854.	*Martin Barry, Phil. Trans.* 1839.
" Many physiologists have felt the incongruity of the comparison generally made between the minute and simple ovum of the mammifer, and the large and more complex yolk of the bird, and most are disposed to acknowledge the necessity of making some more marked distinction between the granular and the cellular yolk substance in the two great groups to which these ova respectively belong. It has before been stated, that von Baer, on his discovery of the mammiferous ovum, regarded it as corresponding, not to the whole	After showing that cleavage, so called, takes place in the ovum of the highest class, Mammalia, I remarked : " In Fishes the divisions in question do not include the whole yelk-ball, but are confined to a projection on one side. In the Frog those divisions include the whole of the yelk-ball, but they begin and proceed more vigorously on one side. In Mammalia also, as we have seen, the divisions include the whole of the so-called ' yelk-ball.' This comparison may perhaps assist us in determining what portion it is of the

* The said drawings A to E represent the young ovum and ovisac of a Bird—the Wryneck, *Yunx torquilla.* In F is part of a young ovisac of the Canary, *Fringilla Canaria.*

ovum of birds, but to the vesicle of Purkinje. The discovery, in 1834, of the germinal vesicle in the mammiferous ovum, of the existence of which von Baer had no distinct knowledge, induced Valentin and others to maintain that the essential parts of the ovum are the same in the bird and the mammifer. But it may be doubted whether physiologists may not have proceeded further than they were warranted by observation in regarding the vitelline membrane and large corpuscles of the yellow yolk of birds as essentially corresponding parts with the zona pellucida and the smaller granular yolk of the mammifer."

" The minutely granular yolk [of the Bird], in which the cicatricula originates, and the germinal vesicle together are the true representatives of the small ovum of the mammifer." In conclusion it is remarked : " This substance by itself constitutes the whole yolk of mammalia ; but in birds it probably remains as a part, if not the whole, of the proligerous disc (afterwards cicatricula)*."

ova of the animals just mentioned which corresponds to the 'yelk-ball' in Mammalia. Is not the 'discus vitellinus' in the *ovarian* ovum of the Bird the seat of similar divisions? If so, it will perhaps appear that the so-called 'yelk-ball' in the mammiferous ovum corresponds more particularly to the 'discus vitellinus' (with its germinal vesicle) in the ovum of the Bird . . . . If the contents of the ovarian vesicle of Baer correspond to no more than the 'discus vitellinus' in the ovarian ovum of Birds and other animals, the former will not appear to be relatively so minute as hitherto supposed. As to the difference in form of these two objects, perhaps a globular form of the substance composing the 'discus vitellinus' would have been incompatible with its position under the vitellary membrane and with the presence around it—in the ovum of the Bird for instance—of a large quantity of true yelk, provided for a future purpose ; while no such provision being required in the ovum of the mammal, the substance corresponding, as I suppose, to the 'discus vitellinus' of other animals fills the vitellary membrane (f), and is therefore globular in form†."

" In the theoretical deductions " made from his observations, Prof. Allen Thomson remarks that the German author "has not been equally successful." The paper of Dr. H. Meckel I have not seen, and became aware of its existence through the article " Ovum " published in June of the present year : but, judging from the following, quoted by Prof. A. T. from the said paper, I too am of the opinion that in theoretical deductions its author

* Article " Ovum," *l. c.* pp. 76 and 79.

† Researches in Embryology, Second Series, Phil. Trans. 1839, pp. 369, 370. I added at the same time in a note (p. 370) : " If the analogy in question really exists, the 'discus vitellinus' is obviously a nucleus destined to undergo changes like those occurring in the so-called 'yelk-ball' of the Mammalia. The round white spot called the 'cicatricula' in the Bird's *laid* egg may possibly correspond to my layer of 'cells,' plate 6. figs. 111–113, lining the vitellary membrane (f) in the *uterine* ovum of the Mammal; while my 'mulberry-like object,' in the same figures, may perhaps be represented in the Bird's *laid* egg by the structure which lies under the 'cicatricula,' and has been denominated Keimhügel, cumulus proligerus," &c.

is not free from error. " I believe, therefore," says H. Meckel, " that that alone ought to be regarded as the true egg which exists in Man, Mammalia, Naked Amphibia and Osseous Fishes; and that in the remaining Vertebrata the ovum consists only of the so-called vesicle of Purkinje, and that all the other parts are accessory, superimposed and unessential. In particular that the yellow yolk of the bird and scaly reptile is analogous to the corpus luteum of the human ovary " [!], &c.

Prof. Allen Thomson seems to think, with H. Meckel, that the zona pellucida in Birds disappears, that the "yellow yolk" is then added, and that around the yellow yolk a membrane is afterwards formed, which is the vitelline membrane of the Bird's laid egg. I have entered upon no renewed inquiries with the microscope upon this subject, but must say that I did not observe any such disappearance of the original zona in the ovarian ovum of the Bird, and have always considered the vitellary membrane of its laid egg to be the original one in a distended state, it having imbibed the material for the superadded true yellow yolk.

XXXVIII. *Some account of the Discoveries of* Keber *on the Porosity of Bodies ; with Confirmations. By* MARTIN BARRY, *M.D., F.R.S.*[*]

A FTER reading the work of Keber on the porosity of bodies, a copy of which the author was so good as to send me immediately after its publication, I felt that English physiologists would be glad to see an account of the same, and thus be afforded an opportunity of testing the observations it records. I therefore wrote to Dr. Keber, stating, that if he were inclined to send me a short abstract of the work, I would, my health permitting, translate and send it to one of our journals. The following is the result—in communicating which I have the satisfaction of knowing it to be the opinion of the illustrious Owen, that the said work has " peculiar value," from its indefatigable author having devoted his skill " to a department of research of prime influence at the actual phase of the physiology of tissues."

Microscopic Researches on the Porosity of Bodies.
By F. KEBER.

" Under this title I have recently published a paper[†] containing the results of microscopic researches continued for months.

After the numerous proofs furnished by experiment as well as through logical conclusions, all acquainted with physics and physiology had long considered the porosity of solid bodies as an established fact.   My object was to give *optical* demonstration of the same.   Of the methods I pursued, and the results thereby obtained—which to physiology and the practice of medicine may not be unimportant—the present communication is intended to give a short outline.

That porosity is a universal quality of bodies, *i. e.* that bodies do not entirely fill the space they take up, but enclose interstices between their particles, is properly adopted in physics as an established fact.   Any compendium of physics may be referred to in proof of this.   To cite merely one of the most striking examples, I remember an experiment instituted by the Academicians of Florence in 1661.   A hollow sphere of gold having been filled with water, was exposed to strong pressure, when its whole surface became covered with minute drops, like those of dew.   Since then the experiment has often been repeated with different metals, and always with the same result.   Now, gold being one of the densest bodies, still less can the finely porous structure of others be filled up.   Thus it is known that stones which have lain for a long time at the bottom of rivers or the sea, are in their interior more or less moist.

The porosity of all animal and vegetable textures follows from their name; for as every texture consists of a network of variously twisted threads or filaments, the organic parts of textures must consequently have interstices.   That such is really the case, daily experience shows.   Thus wood steeped in water increases in weight and volume; while, on the contrary, wood lying in the air, with dry weather dries up, and with wet weather swells.   Further, by means of atmospheric pressure, mercury may be pressed through dense woods, &c.

What especially concerns animal membranes,—the membrane of the cell, the epidermis, the mucus and vascular membranes,—is the penetrability of the same for gases and liquids; which, since the application by Dutrochet to the process discovered by Parrot and Fisher of the expressions Endosmose and Exosmose, has been an established and universally adopted axiom.   Whether by these expressions is to be understood, as by most natural philosophers, merely imbibition and diffusion, the effect of capillary attraction, or whether peculiar properties are to be vindicated for them, which it has lately been attempted to deduce from the chemical nature and the mechanical constitution of the membranes themselves[*], is for our purpose a matter of

* Justus Liebig, *Untersuchungen über einige Ursachen der Säftebewegung im thierischen Organismus*, 1848, pp. 58, 59.

indifference. This much is certain, modern physiology so far adopts a porosity of the organic membranes, as to admit that these are permeable for liquids and gases, but not to allow the passage through them of *any solid* substance. Thus, with reference to the latter, the following enunciation has been given by Johannes Müller:—" The appearance of globules in secretions, presupposes formation of the same at the moment of separation. They cannot pass out of the blood through the capillaries*." Further, it is said by Henle in his treatise on 'Eiterinfection,' " I regard the vascular wall as a hermetically closed membrane, which keeps at a distance from the blood the exciters to putrefaction—microscopic organisms†."

The full validity of these enunciations has, however, recently been called in question, through various physiological and pathological facts, which deserve the greatest attention, although no one has hitherto been able to find for them sufficient explanation. To these belongs, in the first place, the penetration of the fat globules into the lacteals and blood-vessels, concerning which it has with tolerable certainty been shown, that they as such get through the intestinal walls without previous chemical change‡. Further, may be mentioned what experience shows to take place, the penetration, in the form of minute globules, of the mercury contained in blue ointment through the skin into the humours, whereof salivation and other symptoms afford proof to every physician. The powerfully healing effects of the rubbing in of lard and other fatty ointments, do not well admit of being otherwise explained. Now, after the passage of metallic mercury into the blood of animals—partly rubbed into uninjured parts of the skin, and partly given them in food—had been microscopically shown, which was first done by Osterlen§, who arrived at the same result in his experiments with carbon, these observations were most fully confirmed by Eberhardt‖, Mensonides¶, and Donders**, and extended to the penetration of particles of sulphur and starch-granules. So that consequently the possibility of the penetration of finely-divided substances

* *Handbuch der Physiologie des Menschen*, 4th edit., vol. i. p. 202.

† *Handbuch der rationellen Pathologie*, vol. i. 1847, p. 767.

‡ F. Bidder and C. Schmidt, *Die Verdauungssäfte und der Stoffwechsel*, 1852, p. 250.

§ *Archiv für physiol. Heilkunde*, 1843, p. 536; and *Zeitschrift für rationelle Medizin*, vol. v. p. 434.

‖ Henle's and Pfeufer's *Zeitschrift für rat. Medizin*, New Series, vol. i. 1851, p. 406.

¶ Aldus Mensonides, *De absorptione molecularum solidarum nonnulla.* Traj. 1848.

** *Nederlandsch Lancet*, vol. iv.; and *Zeitschrift für rat. Medizin*, 1851, p. 415.

into the humours can be no longer seriously doubted. Lastly, the circumstance that, after its application as blue ointment, mercury in the metallic form has been found, not only in many parts of the body, but even in the saliva*, is evidence of a very general porosity of the animal formations.

To meet these inexorable postulates of logic and experimental proof, science in its present position gives us on this exceedingly important subject no satisfactory disclosure. In vain do we seek in compendiums of physics for definite specifications of the size, form, and condition in other respects, of the pores of gold through which, in the experiment above mentioned, water is pressed out, or of the pores of granite through which the latter in water becomes saturated. Still less is modern physiology in a condition to mark with certainty the ways in which those substances get from the surface of the skin into the blood. While, through the united efforts of the greatest physiologists of the age, we have at length advanced so far as to see laid aside the *viæ clandestinæ*† of former days, through which it was sought to explain the passage into the urine of substances introduced into the stomach, suspicion from another side shows itself anew, that the animal membranes, and especially the capillary walls, notwithstanding their undoubtedly closed state, may either everywhere, or at certain parts, possess apertures which have defied the methods of examination hitherto employed, and by which minutely divided solid substances are let through.

But the permeability of organic membranes for gases and fluid substances also, has nothing in modern science to support it besides the grounds of induction, it having hitherto with certainty been *optically* demonstrated by none. In proof of the correctness of these assertions, I refer to what has been enunciated by some of the greatest authorities now living ; for instance, by a Johannes Müller‡, Valentin§, R. Wagner‖, Henle¶, Liebig**, and others, from whose accordant evidence it follows, that the permeability of animal membranes for gases, liquids, and even under particular circumstances for finely divided solid bodies, is in physiology an established fact, yet

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* In the saliva of a child in scarlatina, treated with blue mercurial ointment on account of swelling of glands of the neck, single globules of mercury, having a diameter of $\frac{1}{2000}'''$, were, as moderate salivation appeared, also noticed by myself.

† See J. Müller, *Handb. der Physiol. des Menchen*, vol. i. 1844, p. 197.

‡ *Handbach der Physiologie des Menschen*, vol. i. 1844, pp. 193, 194.

§ *Lehrbuch der Physiologie des Menschen*, vol. i. 1844, p. 63.

‖ *Lehrbuch der Speciellen Physiologie*, 2nd edit., 1843, p. 206, § 171, note 2.

¶ *Rationelle Pathologie*, vol. ii. 1847, p. 145.

** *Untersuchungen über einige Ursachen der Säftebewegung*, 1848, p. 4.

morphologically requires further clearing up, inasmuch as the apertures in the substance of the textures necessarily assumed, from the occurrences above mentioned, could not until now be demonstrated.

After many fruitless attempts, I have been so fortunate as to discover in the substance of all organic bodies already formed, microscopic spaces from $\frac{1}{1000}'''$ to $\frac{1}{4000}'''$ (Paris line) in diameter, and generally in all the bodies which I have examined, to recognize signs of an optically demonstrable and measurable microscopic porosity. Referring, then, to my more ample representation given in the above-mentioned paper, and to the delineations which accompany it, I beg leave here to offer a short description of the simple methods followed by me in my researches, and of the results.

While reflecting on the difficulties which, notwithstanding the perfection of modern microscopes, opposed themselves to the attainment of the long-striven-for end in question, I feared most of all that, with our present means, the objects to be examined could not be finely enough divided to make the spaces between their particles plain. Manifold as had been the searches after pores, there was nothing to make it probable that their perforations were in a perpendicular direction. It seemed much more likely that they ran in directions the most varied, of the sloping and oblique, perhaps even zigzag, for it had been suggested by Henle*, that the pores of the epidermis " might perforate the skin obliquely," and on this account be imperceptible. Further, it was clear that even a thin animal membrane, but which consisted of several layers of different fibres, forming a texture far more perfect than the artificial ones wrought by the hand and machines of man, must indeed have spaces between its fibres; but that so long as the different layers of that texture ran over and through one another, it would be difficult with certainty to discern those spaces. So that, although having at command even the best illumination, and with a microscope of the first order, he who might attempt to search out the pores of an animal membrane, would find himself going astray just as much as one who endeavours to see through a board; for both consist of variously twisted fibres covering one another, the spaces between which nothing but very minute division can bring into view.

But further, as all bodies by no means possess a fibrous structure, and as even organic fibres, so far as they undergo change of material and through moisture swell and are filled out, must likewise *themselves* be porous, it was evident that through perception of the spaces between the fibres the porosity

* *Allgemeine Anatomie,* 1841, p. 237.

of bodies in general would not be shown, and consequently the problem before us would be far from being solved.

As I further meditated thereupon, in what way I might succeed in dividing the subjects of observation to a degree of minuteness that should answer the requirements above mentioned, I concluded to select Nature herself as a guide, and first to consider more intimately such objects as always present themselves in a minutely divided state without artificial aid. Among the first to be considered are undoubtedly the delicate particles of dust, which, in consequence of the gradual weathering of solid bodies, come to hover in the air, and are deposited upon all bodies and everywhere in our dwellings. Now Nature being, as the microscope daily shows us, often greatest in the most minute things, and solid bodies being gradually weathered into dust, the said particles of dust appeared to be just a product of the most minute division of Nature's own preparing, in delicacy such as could scarcely be exceeded, or even reached by art. I therefore devoted myself to the examination of glass, which, after lying from twelve to fourteen hours uncovered, had become *slightly dusty*. This I did *without adding water*, and *without* a covering of glass, under the aplanatic eye-piece of my large microscope by Schiek, with 200 and 300 times linear magnifying power. The addition of water I omitted, in order not to produce a swelling of the delicate dust particles, and a change in the volume of their pores. The larger dust particles, whose form and compass were most manifold and irregular, it is true, appeared to me almost entirely untransparent, and only at their borders, here and there, diaphanous. On the contrary, in many of the *most minute*, which likewise presented varied forms, I perceived, on close examination, exceedingly minute spaces and clefts, having a diameter of about the $\frac{1}{1200}'''$ to the $\frac{1}{1300}'''$ (Paris line), the colour of which, however, principally with bright illumination, mostly exhibited a reddish or greenish tinge. With long examination, I noticed that the borders also of the dust-particles, as well as their finer indentations and inlets, frequently presented *exactly the same* reddish and greenish edges, and hence concluded that the colouring of those spaces might be owing to interference and flexion of the light, the modifying influence of which on illumination was known to microscopic observers long since. This accordance in the colouring of the borders, with that of the apparent spaces in the substance of the dust-particles (which, especially with strong illumination, was often observable also when the microscope was placed at a very considerable distance from the window), could not in my opinion do otherwise than serve as a confirmation of my belief that I here had before me real spaces and minute orifices, even had I upon other grounds

been able to mistrust the aplanatic eye-piece of my microscope. A real doubt as to the objective nature of those spaces, moreover, after long and repeated occupation with the same object, could not last longer, as with alternate elevation and lowering of the minute objects, and with alternately increased and diminished illumination, I distinctly saw the light flash through them; and even in examining the larger dust-particles, could follow the spaces *in all gradations,* from complete opacity through the different degrees of the diaphanous to entire transparency. Although, therefore, I was at first much inclined to take what I saw for an optical illusion, long examination compelled me, entirely to abandon such a thought,—ever convincing me anew that porosity is a constant formation common to all dust-particles without exception, and that the pores in the larger of them are either wholly or in part covered, through the lying one upon another of several lamellæ and scales.

This discovery prompted me to examine other organic and inorganic bodies also in a like manner, *i. e. without the addition of water,* and in as minutely divided a condition as possible. I took, for example, a morsel of dried bladder of the hog, and holding the same over a clean glass, freed from all dust, scraped most gently on it with a sharp knife, so that extremely small portions, *scarcely visible with the naked eye,* fell therefrom upon the glass; covered these with glass, likewise cleansed from all dust, in order to avoid any confounding with other particles, and then under the microscope sought out the smallest of the said scrapings. The porosity of these appeared with a distinctness just the same as in the minutest dust-particles, in cases where the scrapings had been *made with sufficient minuteness,* and where of these again only the minutest were selected for examination.

In the larger scrapings also, where *their too great thickness* did not render them opake, I was at any time able with certainty to recognize the higher or lower degree of the diaphanous, as well as a loose, richly spaced structure, in their whole extent; consequently distinct traces of porosity.

Since then I have with perfect certainty recognized, as well individual microscopic pores, as also generally a highly porous structure, not only in all the organic, but also in all inorganic bodies, access to which was possible, even granite and the metals, and, as will be more amply stated further on, have with the micrometer measured the pores.

The methods therein followed I now put together in a few words.

XXXIX. *On the bearing of the Barometrical and Hygrometrical Observations at Hobarton and the Cape of Good Hope on the general theory of the Variations of Atmospherical Phænomena. By* Professor Dove *of Berlin**.

I HAD hoped to have prefaced this volume with a discussion of the meteorological observations made hourly at Hobarton from January 1841 to September 1848 (of which the abstracts were published in 1850 in the first volume of the Hobarton Observations), from the pen of Professor Dove, who had kindly undertaken, at the magnetical and meteorological conference at Cambridge in 1845, to participate to that extent in the reduction and application to theoretical conclusions, of the results of the Observations at the British Colonial Observatories; but M. Dove's appointment, on the death of Professor Mahlmann in November 1848, to the charge of the meteorological observatories in the Prussian states has materially abridged the time at that gentleman's disposal, and he has found himself unable to complete the discussion he had undertaken for the present volume without occasioning an inconvenient delay in its publication; the discussion will therefore be prefixed to the fourth volume; but in the meantime Professor Dove has kindly furnished for this volume the subjoined remarks (written in German) upon the bearing which the barometrical and hygrometrical observations, at the Colonial Observatories at Hobarton and the Cape of Good Hope, have had on the general theory which professes to explain the physical causes of the variations which we observe in the atmospherical phænomena of the globe. The testimony borne by so eminent a meteorologist to the importance and value of this portion of the observations made at the British Colonial Observatories, cannot fail to be highly acceptable to the Government which instituted it, and to the public who have paid for these establishments, as it must be most satisfactory to the officers and to their assistants, by whose patient and unremitting labour facts of which the importance is thus recognized have been added to the foundations of meteorological science. The generalization in which M. Dove has applied them is remarkable alike for its extent and its simplicity, and I am glad of the opportunity of enriching this volume with so interesting a document.

Woolwich, March 17, 1853.   EDWARD SABINE.

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* From " Observations made at the Magnetical and Meteorological Observatory at Hobarton, in Van Diemen Island," vol. iii. Introduction.

The establishment of meteorological stations in distant parts of the globe had, generally speaking, for its immediate object, so to complete the partial knowledge we already possessed of the phænomena over a considerable portion of its surface, as to enable us to take a general view of their course over the whole globe; the result of those endeavours has even exceeded what was hoped for, as besides the information obtained respecting regions where our knowledge was most defective, fresh light has been thrown on those with which we had supposed ourselves already completely acquainted.

Meteorology commenced with us by the study of European phænomena, and its next principal extension was to phænomena observed in the tropical parts of America. If what is true of Europe were equally true of the temperate and cold zones of the earth in all longitudes, and if tropical America in like manner afforded a perfect example of the tropical zone generally, it would be of little consequence where the science of meteorology had been first cultivated; but this is not the case, and a too hasty generalization has led to the neglect of important problems, while others less important have been regarded as essential and placed in the foremost rank. It was necessary that the science should be freed from these youthful trammels, and this needful enfranchisement has been effected by the Russian and by the English system of observations. Russia has done her part in freeing the meteorology of the temperate and cold zones from impressions derived exclusively from the limited European type; and England, which by its Indian stations had undertaken for the torrid zone the same task of enlarging and rectifying the views previously entertained, has besides, by its African and Australian stations (Cape of Good Hope and Hobarton), opened to us the southern hemisphere, and first rendered it possible to treat of the atmosphere as a whole. I will now endeavour to show the importance of being enabled to take such general views, selecting as an example the annual variation of the barometer.

The study of the *annual* barometric variation had long been singularly neglected, while the *diurnal* barometric variation had had devoted to it an attention quite disproportioned to its subordinate interest in reference to the general movements of the atmosphere. This otherwise incomprehensible mistake is excused by the localities where nature had been first interrogated. As the diurnal variation had manifested itself with great distinctness and regularity in tropical America, it naturally presented itself as an object of interest in Europe also. The annual variation, on the other hand, is inconsiderable, both in Europe and the tropical parts of America; and thus, while atmospheric phænomena were treated simply as facts of which the periodicity alone

was to be investigated, without seeking for physical causes, it was natural that a phænomenon, in which opposite effects resulting from two different causes counterbalance each other, should altogether escape notice. It is, perhaps, more remarkable that no surprise should have been excited when the atmospheric pressure was not found to diminish from winter to summer, with increasing heat

When, by the labours of Prinsep more particularly, the phænomena of the tropical atmosphere in Hindostan became more known, there was seen to be a great difference between the barometric variation there and in tropical America; inasmuch as the Indian observations showed a decidedly well-marked annual variation. A new error was now fallen into, and it was supposed that the phænomenon did not extend beyond the torrid zone, and that it was an immediate consequence of the periodical change of wind, *i. e.* of the monsoons. This erroneous view was completely refuted when the barometric relations at the Siberian stations became known; for it was then found, that north of the Himalaya (which in the supposed hypothesis must have formed the limit of the phænomenon), the annual barometric variation was exhibited on a large scale, and over a region so extensive, that the shores of the Icy Sea itself could hardly be assumed as its boundary. A greatly diminished atmospheric pressure taking place in summer over the whole continent of Asia must produce an influx from all surrounding parts; and thus we have west winds in Europe, north winds in the Icy Sea, east winds on the east coasts of Asia, and south winds in India. The monsoon itself becomes, as we see, in this point of view only a secondary or subordinate phænomenon.

I have endeavoured to establish the reality of the above phænomenon and its climatological bearings in several memoirs; and I must refer for the numerical values to Poggendorff's *Annalen,* vol. lviii. p. 177; vol. lxxvii. p. 309; and to the *Berichte* of the Berlin Academy, 1852, p. 285. I will here embody the results in distinct propositions, in order to show, in connexion therewith, the importance of the bearing of the Hobarton observations.

1. At all stations of observation in the torrid and temperate zones the elasticity of the aqueous vapour contained in the atmosphere increases with increasing temperature. In the region of the monsoons this increase from the colder to the warmer months is greatest near their northern limit. Hindostan and China present in this respect the most excessive climate. No differences of similar magnitude are found in the southern hemisphere. The form of the curve of elasticity of the aqueous vapour shows, however, a less decidedly convex summit in the region of the monsoons than beyond it, having in that region rather the cha-

racter of a flattened summit or table-land, the elasticity con-
tinuing nearly the same throughout the period of the rainy mon-
soon.   Near the equator the convex curve of the northern hemi-
sphere becomes, first flattened, and then gradually transformed
into the concave curve of the southern hemisphere. In the Atlantic
this transition takes place in a rather more northerly parallel.
In regard to the magnitude of the annual variation, the following
rule appears generally applicable in the torrid zone : the annual
variation is considerable at all places where equatorial currents
prevail when the sun's altitude is greatest, and polar currents
when the sun's altitude is least ; and inconsiderable wherever
the direction of the wind is either comparatively constant
throughout the year, or where it changes in the contrary sense
to that above described.   At the last-named class of places the
rate of decrease in the mean annual tension of the aqueous
vapour with increasing distance from the equator is more rapid
than in the first class.

    2. At all stations in Europe and Asia the pressure of the dry
air decreases from the colder to the warmer months, and every-
where in the temperate zone has its minimum in the warmest
month.

    3. If we compare the annual variation of the pressure of the
dry air in northern Asia and Hindostan with the variation in
Australia and the Indian Ocean, we shall be satisfied that some-
thing more takes place than a simple periodical exchange of the
same mass of air in the direction of the meridian, between the
northern and southern hemispheres.   From the magnitude of
the variation in the northern hemisphere, and the extent of the
region over which it prevails, we must infer that at the time of
diminished pressure a *lateral* overflow probably takes place ; that
it actually does so may be considered as proved for the northern
part of the region, by the fact that at Sitka, on the north-west
coast of America, the pressure of the dry air *in*creases from winter
to summer.   It is not probable that the overflow takes place
exclusively to the east, it probably occurs also to the west ; and
on this supposition the small amount of the diminution of the
pressure of the dry air from winter to summer in Europe would
be caused, not solely by the moderate amount of the difference
of temperature in the hotter and colder seasons, but also by the
lateral afflux of air in the upper regions of the atmosphere tend-
ing to compensate the pressure lost by thermic expansion.   As
at the northern limit of the monsoon, at Chusan and Pekin, the
annual variation of the pressure of the dry air is most consider-
able, while at the northern limit of the trade wind in the Atlantic
Ocean, *i. e.* at Madeira and the Azores, it is very small, it is pro-
bable that there is in the torrid zone also a lateral overflow in the

upper strata of the atmosphere from the region of the monsoons to that of the trades.

4. From the combined action of the variations of the aqueous vapour and of the dry air we now derive immediately the periodical variations of the whole atmospheric pressure. As the dry air and the aqueous vapour mixed with it press in common on the barometer, so that the upborne column of mercury consists of two parts, one borne by the dry air, the other by the aqueous vapour, we may well understand that as with increasing temperature the air expands, and by reason of its augmented volume rises higher and at its upper portion overflows laterally,—while at the same time the increased temperature causes increasing evaporation, and thus augments the quantity of aqueous vapour in the atmosphere,—so it naturally follows that the composite result in the periodical variations of the barometric pressure should not everywhere bear a simple and immediately obvious relation to the periodical changes of temperature. It is only when we know the relative proportions of the two variations which take place in opposite directions that we can determine whether their joint effect will be an increase or a decrease with increasing temperature,—whether in part of the period the one variation may preponderate and in other parts the other variation. The following are the results which we are enabled to derive from observation.

5. Throughout Asia, the increase in the elasticity of the aqueous vapour with increasing heat is never sufficient to compensate the diminished pressure of the dry air, and the annual variation of barometric pressure is therefore everywhere represented, in accordance with the variation of the pressure of the dry air, by a simple concave curve having its lowest part or minimum in July. The observations in Taimyr Land, at Iakousk, Udskoi and Aiansk, show that this is true up to the Icy Sea on the north, and to the sea of Ochotsk on the east. On the west a tendency towards these conditions begins to be perceived in European Russia in the meridian of St. Petersburg, and becomes more marked as the range of the Ural is approached. On the Caspian and in the Caucasus the phænomenon is already very distinctly marked; its limit runs south from the western shore of the Black Sea, so that Syria, Egypt, and Abyssinia fall within the region over which it prevails. Towards the confines of Europe there is almost everywhere a maximum in September or October, the barometric pressure increasing rapidly from July to the autumn. This maximum is followed towards the latter part of the autumn by a slighter inflexion or secondary minimum; it is only beyond the Ural that the curves become uniformly concave, with a single summer minimum and winter maximum,

which character they retain throughout the rest of the Asiatic continent, even to its eastern coast. In winter the absolute height of the barometer at the northern limit of the monsoon is very great. The still considerable amount of the annual variation at Nangasaki, and the little difference between the curve of Manilla and that of Madras, show that the region in question extends beyond the eastern coast of Asia into the Pacific Ocean; in higher latitudes, however, its limits appear to be reached in Kamschatka. As the annual variation, which is greater at Madras than at Manilla, is found greater at Aden than at Madras, the western limit of the region would appear to extend far on the African side.

6. In middle and western Europe the barometric pressure appears to decrease everywhere from the month of January to the spring, usually attaining a minimum in April; it then rises slowly but steadily to September, and sinks rapidly to November, when it usually reaches a second minimum. In summer, therefore, the whole atmospheric pressure gains more by increased evaporation than it loses by expansion. This over-compensation is probably, as we have seen above, to be explained by the lateral overflow received in the upper regions from Asia. In Sitka the whole annual curve is convex, a result only found in Europe at considerable mountain elevations, where it is a consequence of the expansion, and extension upwards, of the whole mass of the atmosphere in summer.

7. The region of great annual barometric variation, on the Asiatic side of the globe where *monsoons* prevail, extends much further to the north in the northern hemisphere, than it does to the south in the southern hemisphere; for the variation reaches its maximum at Pekin, while at Hobarton, in nearly a corresponding latitude, it has already become inconsiderable; and it is generally greater in the northern than in the corresponding southern latitudes. The exact contrary is the case on the Atlantic side and in the region of the *Trades*; for here the annual variation, though nowhere very considerable, is decidedly greater in the southern than in the northern hemisphere, as is shown by the results of observation at the Cape, Ascension, St. Helena, Rio Janeiro, and Pernambuco, compared with the West Indian Islands and the southern parts of the United States. Hence it follows, that if we compare places in the same latitude, we find but little difference between the annual variation in the southern Atlantic and southern Indian oceans, while in the northern hemisphere we have in the same latitude the very large annual variation in the north part of the Indian and in the Chinese seas, and the almost entire absence of annual variation in the Atlantic (compare Chusan with the Azores and Madeira).

The explanation of the last-named phænomenon, *i. e.* that of the northern hemisphere, by a lateral overflow in the upper parts of the atmosphere, seems so direct, that I think we may pronounce the irregular form of the annual barometric curve in the West Indies to be a secondary phænomenon, the primary causes of which must be looked for on the east.

8. It is known that in the eruption of the Coseguina on the 20th of January, 1835, when the isthmus of Central America was shaken by an earthquake, not only were volcanic ashes carried to Kingston in Jamaica, a distance of 800 English miles in the opposite direction to the trade wind, but some of the same ashes also fell 700 miles to the *westward*, on board the Conway, in the Pacific Ocean. We infer, therefore, that in the higher regions of the atmosphere in the tropics the air is not always flowing regularly from S.W. to N.E., but that this usual and regular direction is sometimes interrupted by currents from east to west. I think I have indicated the probable cause of such anomalous currents in the above described barometric relations of the region of the monsoons compared with that of the trades. If we suppose the upper portions of the air ascending over Asia and Africa to flow off laterally, and if this takes place suddenly, it will check the course of the upper or counter current above the trade wind, and force it to break into the lower current. An east wind coming into a S.W. current must necessarily occasion a rotatory movement, turning in the opposite direction to the hands of a watch. A rotatory storm moving from S.E. to N.W. in the lower current or trade, would in this view be the result of the encounter of two masses of air impelled towards each other at many places in succession, the further course of the rotation (originating primarily in this manner) being that described by me in detail in a memoir " On the Law of Storms," translated in the Scientific Memoirs, vol. iii. art. 7. Thus it happens that the West India hurricanes and the Chinese typhoons occur near the lateral confines on either side of the great region of atmospheric expansion, the typhoons being probably occasioned by the direct pressure of the air from the region of the trade winds over the Pacific into the more expanded air of the monsoon region, and being distinct from the storms appropriately called by the Portuguese " Temporales," which accompany the outburst of the monsoon when the direction of the wind is reversed. The fact of the rotatory storms being of much more rare occurrence in the South Atlantic Ocean arises from the more equal distribution of the periodically diminished atmospheric pressure in the southern as compared with the northern hemisphere. Here, therefore, the rotatory storms take place principally in the monsoon itself.

9. It is evident that the unsymmetrical distribution of land and sea, which gives rise to the abnormal variations in the forms of the isothermal lines, is at the same time the principal cause of the movements of the atmosphere. Thus the monsoon is but a modification of the trade wind, of which the cause is to be sought in part beyond the tropic. The region of great thermic expansion of the air in summer in the interior of the continent of the Old World presents all the characteristic marks of the region of calms, being a centre towards which all adjacent masses of air are drawn. Hence there is no complete sub-tropical zone, in the sense of a zone encompassing the globe. The region over which the heated air ascends does not therefore move up and down, or north and south, parallel with the sun's change of declination, but has rather a kind of oscillatory movement, in which the West Indies represent the fixed point, and the greatest amplitude of oscillation is on the side of India. The northern excursion is much greater in the northern hemisphere than is the southern excursion on the side of the southern hemisphere. The European atmospheric relations, especially in summer, are therefore essentially of a secondary nature; and we must regard the little alteration in the atmospheric pressure in the course of the year in Europe as a secondary result, of which the explanation would not have been possible without the observations from Asia and Australia.

Berlin, January 5, 1853.

XL. *On the May Hill Sandstone, and the Palæozoic System of England. By the* Rev. Prof. Adam Sedgwick, *F.R.S., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE following paper was drawn up for the Geological Society of London during last October, in the hope of its being read at their first Meeting after the summer recess. I was, however, too late in my application; and in consequence of a long protracted illness, I was never able to attend a single meeting of the Society during the past winter or during the spring of this year. The paper was consequently postponed, and at length read, in my absence, at one of the concluding spring Meetings of the Geological Society. The Council are ready to print the paper—suppressing the discussions on classification and nomenclature—in their Quarterly Journal. I believe that any such suppression would destroy the value of the paper; and I offer it to you as it was originally drawn up, with the following exceptions. One of the diagrams (fig. 4) has been corrected—

the " Tabular view " of the Devonian, Carboniferous, and Permian series has been slightly expanded, and a few short explanatory notes have been added. In other respects, it is, with the mere exception of verbal corrections, word for word as it was submitted to the Geological Society.

<div style="text-align:center">I have the honour to be, Gentlemen,</div>

<div style="text-align:center">Your faithful Servant,</div>

<div style="text-align:right">A. SEDGWICK.</div>

Cambridge, August 20, 1854.

This paper is a continuation of one which was read before the Geological Society (Nov. 3rd, 1852), and was afterwards published in their Journal, vol. ix. p. 215. The results stated by Prof. M'Coy and myself in that paper were as follows :—

1st. The May Hill sandstone contains a group of fossils essentially distinct from those which are found in the sections of Horderley and Caer Caradoc. Its fossils are of a true Wenlock type ; and it must consequently be cut off from the Caradoc sandstone, and arranged as the base of the Wenlock group.

2ndly. In the great series of beds on the south-western flank of the Malvern Hills, more than 2000 feet of strata, which had been called Caradoc sandstone, were separated into two distinct groups :—the *lower* (Hollybush sandstone and Black shale) being provisionally called Caradoc sandstone and shale; the *upper* group being the exact equivalent of what we called the May Hill sandstone. Adopting the same nomenclature, we found in the more northern sections, on the west flank of the Malverns, no traces of the Caradoc (or Hollybush) sandstone.

It was further stated (*loc. cit.* p. 228), that during a former visit to the Horderley section (made in 1842) I had found beds of shale, with *Trinucleus, Caractaci*, &c., immediately above Stretford Bridge. Now, if this were true, the well known Pentamerus or Hollies limestone must be overlaid by a true Caradoc shale; and the ascending order of succession must be (as stated in the paper referred to)—" (1) Caradoc sandstone, ending with the Pentamerus or Hollies limestone ; (2) Caradoc shale ; (3) Wenlock shale ; (4) Wenlock limestone," &c. This conclusion was strenuously opposed by Professor M'Coy ; but we had no opportunity, in 1852, of bringing it to the test. It was derived from an old note-book ; and as it made *against myself*, I thought myself bound to state it. Were the conclusion true, it would only prove, that on the banks of the Onny (as in several sections east of the Berwyn chain) the Wenlock shale is brought by an unconformable overlap, into immediate contact with the true Caradoc group, and without the intervention of the May Hill

sandstone *. I may further state, that during two short visits to the Onny section, made by my friend John Ruthven and myself, in the summers of 1846 and 1851, for the sole purpose of collecting fossils, we did not find a single characteristic Cara-doc species over the Hollies limestone. The state of the river had on both occasions been very unfavourable to our examination of the shales near Stretford Bridge; but my friend Mr. Duppa, of Cheney Longville, promised (in 1851) to make excavations among them, in places where they were out of the reach of the waters. He has since then amply performed his promise, and his excavations have not laid bare any characteristic Caradoc species in the shales immediately above the bridge. I therefore now accept a suggestion (more than once made by Mr. Salter and Prof. M'Coy), that, in my note-book of 1842, I had mistaken Stretford Bridge for another bridge further up the Onny; and in this way the section of the Onny is no longer (as it will ap-pear in the sequel) in any real antagonism with the sections of May Hill and the Malverns.

These preliminary remarks will sufficiently explain the pur-pose of our short visit, made near the end of August (1853), to the frontiers of Wales and Siluria.

My first intention was to re-examine (during the early part of last summer) the grits, conglomerates, and shelly sandstones which range from Conway to the neighbourhood of Corwen, and form the base of the Denbigh flagstone: and I may remark, by the way, that these important beds had been carefully laid down in 1843 by Mr. Salter and myself, and were at that time con-sidered and coloured as Upper Silurian. After having effected this first purpose, I hoped to follow the same grits and sand-stone in their range along the Berwyn chain; and lastly to fol-low them, as they are laid down (I doubt not with great accu-racy) in the Government Map, until they finally thin out and disappear.

I believe that, with very limited exceptions, the whole " Middle Silurian " group of the Government Map is the exact equivalent of the " May Hill sandstone." The conglomerates, grits, and sandstones above mentioned are unconformable to the Cambrian rocks on which they rest, in their range from Conway to Corwen, and, in that part of their range, are the undoubted equivalents of the May Hill sandstone. That they are unconformable to the Cambrian rocks on which they rest, in a part of the Berwyn range, is also, I think, evident; for they appear as the highest beds of a trough, on both sides of which there is an outcrop of

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* I stated this hypothetical conclusion in a letter to my friend Mr. Salter before he commenced his re-examination of the Horderley and Wenlock sections in 1853.

the Bala limestone; and if we make sections from these highest beds to the limestones on the two sides of the trough, we find a discrepancy in thickness which can be readily explained by the discordant position of the overlying mass, and cannot, I think, be explained any other way. I state this fact from my remembrance of the sections made by Mr. Salter and myself in 1843.

Lastly, it is I think clear, from a glance over the map of the Government Survey, that their so-called "Middle Silurian" group is generally unconformable to the older Cambrian rocks on which it rests. It must, therefore, be obvious, that the task I had proposed to myself was immediately connected with the facts and conclusions of my preceding paper (read Nov. 3rd, 1852); but the bad state of my health compelled me, very reluctantly, to abandon the greater part of it.

There remained, however, a second and shorter task in which Professor M'Coy had promised to join me, so soon as he had completed his work in the Cambridge Museum. We proposed to examine in detail the section of Mathyrafal, near Meifod, the sections of the Pentamerus limestone on the flanks of the Longmynd, the sections of the Onny and of Caer Caradoc, and lastly, the sections of Builth, Llandovery, and Llandielo *. I thought that the facts exhibited in these sections must have a direct bearing on the conclusions we had drawn in the preceding year. A very vexatious accident detained us full three weeks beyond the time we had fixed on for the commencement of our task. It was in consequence left incomplete, and our examination of the following sections was confined within the limits of a single week, during more than half of which I was unable to take the field, or to give any help to my fellow-labourer.

I. *Mathyrafal Section.*

This section is seen on the north side of the valley, a few miles above Meifod, and its upper beds are now cut through by the new road (from Meifod to Llanfair) which did not exist (in 1843) when the section was examined by Mr. Salter and myself. I will not enter on details which have been already published in the Journal of the Geological Society, but I wish to recall to memory the fact, that there are at least two calcareous bands in the lower part of this section; and that the upper band, which is concretionary and discontinuous, is associated with a coarse conglomerate and with a hard sandstone, which appears in a nearly vertical position by the side of the new road. These vertical beds are overlaid by highly fossiliferous shales and flagstones of a considerable thickness, which gradually lose their

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* All the above are critical sections, at the junction of the groups I call Cambrian, with the overlying groups which are undoubtedly Silurian.

great inclination and pass under the regular terrace of the Wen-lock shale.

An important negative fact is presented by this section. It gives no trace of the May Hill sandstone; and there is no pass-age between the soft earthy beds of the Mathyrafal group and the beds of Wenlock shale which rest upon them, the upper formation having been brought over the lower by an unconform-able overlap. If, in any single section near the cuttings of the new road, there be the appearance of such a passage, it is, I believe, deceptive.

The same remark I would now apply without hesitation to the mistaken appearance of a passage between the Cambrian rocks of Glyn Ceiriog and the overlying beds of Wenlock shale, as seen a few miles to the south of Llangollen, where, in like man-ner, the May Hill beds are entirely wanting. I may further remark, that when two unconformable deposits are composed of soft yielding materials (like those just alluded to), it is often impossible, on the evidence of one section, to conclude that they are the conformable portions of a regular and uninterrupted sequence. The lower soft and yielding beds, when pressed down by the superincumbent mass, may give a false appearance of conformity, which may become still more deceptive in cases where both the upper and lower masses have been *afterwards* subjected to common movements of contortion. On the con-trary, where the lower beds are hard and inflexible, a discordancy of position becomes at once palpable and obvious. Thus, in the neighbourhood of Welch Pool, we have, between the hard and highly inclined Cambrian rocks (which have, I now think erro-neously, been called Caradoc sandstone), and the overlying flag-stones of the Wenlock age, very obvious cases of unconformity.

In the hope of making these conclusions clear, I subjoin three ideal vertical sections, two of which (figs. 2 and 3) show the actual sequence of deposits to the east of the Berwyn chain (*e. g.* at Mathyrafal and Glyn Ceiriog), while the other (fig. 1) shows a similar sequence, among rocks of nearly the same age, on the west side of the Berwyn chain on the line of the Holyhead road. Of these sections, fig. 1 gives a fine exhibition of the May Hill sandstone overlaid by the Denbigh flags, Wenlock and Ludlow, and underlaid discordantly by a part of the great Bala group. Fig. 2 represents a case like that of Mathyrafal, where the May Hill sandstones and conglomerates disappear, and the Wenlock groups rest, with a slight discordancy of position, upon the Cam-brian rocks, which here belong to a high part of the Bala group. Fig. 3 represents the case of Glyn Ceiriog, where there is no *apparent* discordancy of position, but where the May Hill group is entirely wanting. In one respect all these sections are imper-

fect, inasmuch as not one of them shows a perfect and continuous sequence of all the deposits within the limits of the section, and I have never seen any such perfect section in North Wales*.

Fig. 1.   Fig. 2.   Fig. 3.
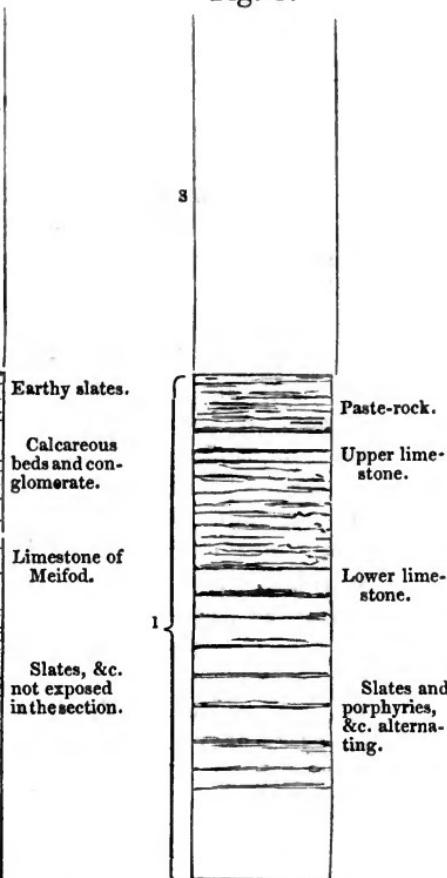
Three sections representing the junction of Cambrian and Silurian rocks in North Wales.

Fig. 1. From the neighbourhood of Cerniogie on the Holyhead road, where the May Hill group (No. 2) is largely developed, and rests unconformably upon a part of the Upper Bala group (No. 1), and is surmounted conformably by the Denbigh flag (Wenlock, &c.) (No. 3).

Fig. 2. From Mathyrafal. Here the Upper Bala group (No. 1) has an unusual mineral type, and is overlapped with a slight discordancy of position by the Wenlock series (No. 3). The May Hill series (No. 2) is entirely wanting.

Fig. 3. From Glyn Ceiriog, south of Llangollen. Here a portion of the Upper Bala group (No. 1) is overlaid, without any apparent discordancy of position, by the Denbigh flag (No. 3); but the May Hill group (No. 2) is entirely wanting.

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* I believe, however, that some rather soft earthy slates (seen at Glyn

II. *Pentamerus beds of Norbury and Linley*.*

The extension of these beds along the southern edge of the Longmynd chain, upon which they rest unconformably, has been well known to geologists since the publication of the 'Silurian System.' In Siluria they were discovered, and there they have their best, and perhaps their only unequivocal type. Their thickness is not great, and at Linley and Norbury the thickness might, on a superficial view, be easily overrated, inasmuch as the beds dip very nearly with the inclination of the ground, and therefore are spread over a considerable surface. It would be idle for me to attempt any detailed description of these well-known beds; but I may just remark,—(1) That some of the lowest beds are very irregular, coarse, and of mechanical structure, and with very few traces of fossils. (2) That over the above are five or six feet of a bluish-gray ragstone: the beds with very uneven upper and lower surfaces, separated by semi-indurated wayboards, marked by many ferruginous stains, and with a few casts of fossils. (3) That higher in the section are five or six feet of gray and yellowish-gray beds, slightly ferruginous, and with many cellular streaks marking the presence of fossil casts; also bands of ferruginous "rotten-stone," with innumerable casts of fossils. As usual, the calcareous matter had quite disappeared from the "rotten-stone" bands. (4) That over the above comes the well-known and beautiful Pentamerus (or Norbury) limestone. This limestone is, in the Norbury quarries, extensively worked for the neighbouring lime-kilns. The beds are very irregularly deposited, but their average thickness is about five or six feet. The preceding group plunges under the soil, and no higher beds were seen; but there seems to be no doubt that it is almost immediately overlaid (as represented in our best maps) by the Wenlock shale. These facts would not be worth noticing, did they not serve to give a connected meaning to the following account of the fossils we found in the quarries of Linley and Norbury.

Ceiriog, and also in several sections near the road from Llangollen to Ruthin), which I have often called the *paste-rock*, are very nearly at the crown of the whole Cambrian series. They were in 1843 sometimes regarded as " beds of passage " by my friend Mr. Salter and myself. They were not, however, true beds of passage in the places where we saw them, as they were overlaid by the Wenlock shales, without the intervention of the May Hill sandstone.

* This small group is an important member of the palæozoic system, and it deserves a geographical name. It may (from its best locality) be conveniently called Norbury limestone. " Pentamerus limestone " is a bad name for this small group; for a Pentamerus (though of a different species) is also characteristic of another limestone—the Aymestry.

Fossils of the Pentamerus limestone and May Hill sandstone of Linley and Norbury.

First of all, we may remark that we have, especially in some of the upper beds, the *Pentamerus oblongus* and the *Pentamerus lævis* in infinite abundance. Here, indeed (as I believe also at the Hollies and other well-known localities), they seem to have in a great measure supplied the calcareous matter of the limestone. But in addition to these two abundant fossils, and after rejecting many specimens which were too imperfect to give secure results, Prof. M‘Coy determined the following species, the list of which is here given in his own words :—

" *Ptylodictya lanceolata* (as at Dudley).

Palæopora interstincta (common to Cambrian and Silurian rocks).

Favosites multiporatus (do.).

Petraia bina (as in Wenlock limestone and May Hill sandstone).

P. bina, an unnamed species (same as at May Hill).

Encrinurus punctatus (common to Cambrian and Silurian rocks).

Leptæna transversalis (as in the Wenlock limestone of Woolhope, Dudley, and in the May Hill sandstone).

Leptæna euglypha (Wenlock limestone and Dudley limestone).

Orthis elegantula (common to Cambrian and Silurian rocks).

O. pecten (do.).

O. Davidsoni (Wenlock limestone of Walsall and May Hill sandstone).

Spirigerina reticularis (Cambrian, Silurian and Devonian).

Littorina Octavia (same as in Wenlock limestone)."

Upon this list I may remark, that out of fifteen carefully determined species, eight have not yet, so far as I have heard, been found except in the Wenlock group or its equivalents; and that, of the remaining seven species, one ranges up to the Devonian rocks inclusive. The remaining six belong to species *which were already known* to be *common* both to Cambrian and Silurian rocks. And lastly, that all those very abundant types, which have *hitherto* been found *exclusively* in the Bala and other undoubted Cambrian groups, are here entirely wanting. We found no other examples of species common to Cambrian and Silurian rocks. But we were by no means surprised to find several well known common species in a deposit which is at the base of the Wenlock shale. It is the exact locality where we should have expected to find such species.

By itself, this evidence might not be considered sufficient to prove that the *Norbury group* is to be cut off from the Caradoc sandstone, and arranged with the May Hill sandstone. But

this evidence cannot be taken by itself. It it be called doubtful, there is other evidence in reserve which is not doubtful, and which, I think, proves that this Norbury group is an integral part of the May Hill sandstone; that it is unconformable to the Cambrian rocks where the sections are most complete; and that it does form a portion of those important arenaceous and shelly deposits which are, both physically and palæontologically, the true base of all the overlying Silurian groups*.

III. *Sections of Horderley and the Onny.*

We were well aware of the importance of some sections at the northern end of the Longmynd range, and it was part of our plan to visit them. But my health gave way, and Prof. M'Coy was unwilling to leave me; we therefore removed to the neighbourhood of the Onny, and he proceeded to examine the sections above Stretford Bridge. Comparatively little could have been made of them during a period of great inundation, had not our friend Mr. Duppa previously made his promised excavations in one or two places above the water-level, and conducted Prof. M'Coy to them. The evidence was as follows:— About 200 yards above Stretford Bridge, in an excavation made where I had supposed (from my Notes of 1842) that the Caradoc shale extended, they found many Wenlock fossils unmixed with any older types. Prof. M'Coy's notes are as follows:—

"1. In this excavation was an *abundance* of the following species:—

Graptolites Ludensis.	*Calymene tuberculosa.*
Odontochile longicaudata.	*Cardiola interrupta.*

2. "Between this locality and that which follows, is a change of surface; and no rock, as stated to me by Mr. Duppa, is distinctly seen for some hundred yards. This is the place where we might expect to see the May Hill sandstone and the Pentamerus (or Norbury) limestone; but they are lost in the obscurity of the section.

3. "About 200 or 300 yards above Longville Bridge the following fossils are abundant, without any intermixture of the above-named Wenlock species:—

Orthis calligramma.	*Leptæna sericea.*
—— *elegantula* (var. α).	—— *quinquecostata.*
—— *parva.*	

(All the above are Cambrian.)

Spirigerina reticularis (Caradoc to Devonian inclusive).

4. "About thirty yards higher up the river an excavation had

* The reader will observe that *all the groups below the May Hill sandstone* are in this paper considered as *Cambrian.*

been made in a bed of shale, with innumerable specimens of *Trinucleus.*

5. " Still further up the river followed the well-known Caradoc beds of Horderley."

In the above section the highest beds are undoubted Wenlock shale. The lower beds (groups 3, 4 and 5) are undoubted Cambrian (one of the upper sandstones of the Bala group). The intermediate, or May Hill group, is lost.

There is therefore no contradiction in the section to the views given in my previous paper of Nov. 3, 1852. We have no alternation of true Cambrian and true Silurian types; and I now believe that the Trinucleus shale laid bare by an excavation a little above Longville Bridge, was (by an error in my notes in 1842) placed a little above Stretford Bridge. This conclusion I should have come to sooner, had I dared to coax my notes into an agreement with my subsequent views.

IV. *Sections near Shineton through a part of the Caradoc terrace, a few miles to the north-west of Wenlock.*

What I had first thought the most important object of our excursion was to examine the whole Caradoc terrace between the Onny and the Severn, in order that we might learn whether there existed in that district any unequivocal traces of the May Hill sandstone interposed between the true Caradoc beds and the Wenlock shale. I thought it probable when my former paper was written (Nov. 1852), that here (as in the lower part of Glyn Ceiriog) the Wenlock shale *might by an overlap* have been brought immediately into contact with the true Caradoc group. But before we commenced our excursion, we learnt from Mr. Salter, who had just before visited the country near Wenlock, that the previous conjecture was erroneous, and that there did exist at Shineton and other places along the Caradoc terrace, a series of beds which represented the May Hill sandstone. He gave us his best localities, and a short list of the fossils he had collected from them ; and he added his conviction, that these so-called May Hill beds at the base of the Wenlock shale were unconformable to the contiguous parts of the *true Caradoc sandstone.*

While I was still unable to take the field, Prof. M'Coy not only traversed the north-eastern end of the Caradoc terrace, but completely verified the previous observations of Mr. Salter ; and I will give the result of his observations in his own words.

(1.) " On the road-side, close to Shineton Church, are olive-coloured shales which dip about 35° E. of south, and at about 30°. They were found to contain the following fossils :—

Agnostus pisiformis (as at Llandeilo, &c.) in great abundance.

Olenus* ? (same species as at Hollybush,—Malvern sections of Phillips).

Asaphus ?, undetermined fragments.

Cytheropsis Aldensis (as at Aldens on the Stincher, N. B.).

Siphonotreta micula (as at Wellsfield near Builth, and at Pentre, north of Llangynyw).

All the above are Cambrian types.

(2.) " Over these olive shales, and also over some black shales†, with a few traces of *Fuci* and *Orthoceratites* in Belswardine Brook, are several thin beds of May Hill sandstone and Pentamerus limestone. Dip about 50° E. of south, at 20°. The fossils observed were,—

Hemithyris hemisphærica.
Pentamerus lævis.
—— *oblongus.*
Petraia (unnamed species, same as at May Hill and Malvern).

(3.) " One mile west of Harley are olive-coloured shales, like those of Shineton, and with nearly the same dip and strike. They are overlaid (with a small degree of unconformity) by very coarse unfossiliferous May Hill conglomerates; exactly like those which appear at the base of the May Hill sandstone near the top of May Hill. They are seen in numerous openings along the road to Church Green.

(4.) " *Beneath the above conglomerates,* in large quarries near Harnage Grange, the true Caradoc sandstone and limestone are both found, dipping 10° E. of South, at about 20°, and full of the following fossils‡.

Orthis expansa.	*Orthis parva.*
—— *vespertilio.*	—— *Actoniæ.*
—— *elegantula, var. a.*	—— *bilobata.*

All of which are exclusively Cambrian."

Such were Prof. M'Coy's notes. His excursion was made during a day of almost unrelenting rain, otherwise his fossil lists would have been more complete. The above facts, combined

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* This was one of the species in Mr. Salter's list.

† In these black shales (as in those of Holly Bush described by Professor Phillips) have been several ignorant trials for coal.

‡ Professor M'Coy *does not assert* (as I stated by *mistake* to the Geological Society) that the sandstones near Harnage Grange *underlie* the olive-coloured Olenus shales of Shineton. He merely states facts, and leaves in doubt the *exact* sectional position of the shales. But he regarded them as probably forming the base of the Caradoc terrace which extends from Horderley to the Severn. The fossils of the olive-coloured shales evidently indicate their true position, and prove them to be on the same parallel with the beds at Builth and Llandeilo. I was led into the mistake above-mentioned by the shortness of Professor M'Coy's notes, and by finding that Shineton was close to the Wenlock terrace on the Geological Map.

with those previously observed by Mr. Salter, were, however, sufficient for our purpose, and enabled us to draw the following conclusions :—

(1.) There can now be very little doubt about the geological place of the Hollybush Olenus shales. They are where I had provisionally placed them in my last paper; adopting without reserve the published views of Professor Phillips, who first discovered and described them.

Near Shineton, there is nothing in the sections to indicate the protrusion of any very ancient rock ; nor any igneous rock (like the Malvern syenite) to disturb the relations of the neighbouring strata. Hence it appears certain that the Shineton beds (although inferior to the above-mentioned beds of Harnage Grange) must be arranged with the shales which form the base of the Horderley or Caradoc terrace ; in which case they must be subordinate to the Bala group.

The note of interrogation after the word *Olenus* in the previous list may perhaps show that Prof. M'Coy is not certain as to the genus; but, whatever it may be hereafter called, he is certain that it is identical with the *Olenus* of the Hollybush shales.

(2.) The (supposed) typical section through Caer Caradoc, Wenlock Edge, &c., is not a truly continuous, but a broken section. The true Caradoc sandstone and Caradoc shale do not graduate into the beds which immediately surmount them ; and the conglomerates, grits, and Pentamerus limestone, &c. (which discordantly overlie the true Caradoc sandstone) must here, as at the south end of the Malvern Hills, be cut off from the Caradoc terrace and arranged with the Wenlock group.

(3.) From the above facts, and from all I have seen or learnt of the older palæozoic rocks of North and South Wales and the neighbouring English counties, I think I may conclude, that in these great physical regions there is not so much as one continuous unbroken section through which we can ascend, in the way of passage, from the Cambrian to the overlying Silurian groups. There is a physical break between them exactly on the horizon of the May Hill sandstone ; and in very exact co-ordination with that break (sometimes distinctly marked by a discordancy in the position of the beds), there is a great change in the fossil species. If this be true, and I know nothing to oppose to it, we have at length found the true physical and palæontological base of a " Silurian System."

The accompanying section (fig. 4) represents what is, I believe, the true sequence of the deposits in the Caradoc section. Between the Longmynd slate and the overlying slate and sandstone, there is great interval, the filling up of which requires the interpolation of, at the least, 20,000 feet of strata, from the

Fig. 4. *A Section representing the relations of the groups of Caer Caradoc to the overlying Silurian rocks.*

Fig. 4.
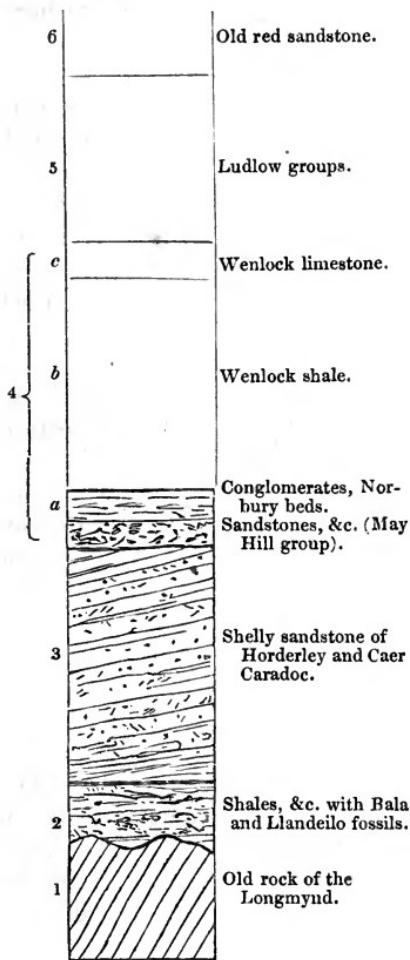
Fig. 5.
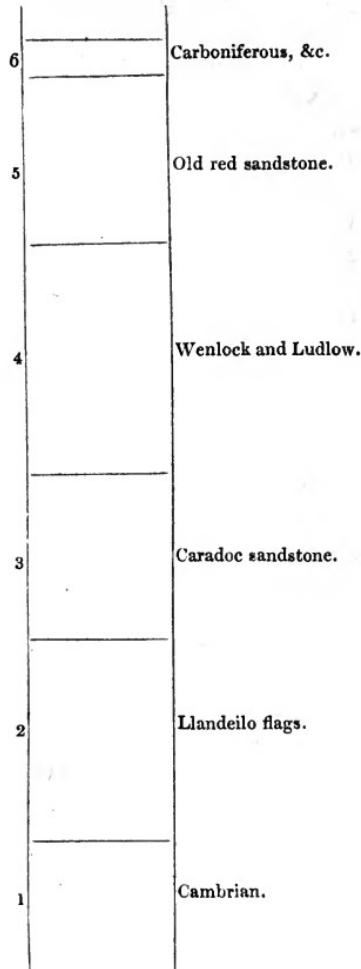
In the section fig. 4 (from the neighbourhood of Caer Caradoc), No. 4*a* is, both from its position and fossils, separated from No. 3 and grouped with No. 4 (Wenlock). In the ' Silurian System ' Nos. (4*a*) and (3) were erroneously placed in one group—the " Caradoc sandstone." The group (4*a*) is here regarded as the true physical and palæontological base of the Silurian series.

This vertical section (fig. 5) represents the supposed sequence of deposits near Llandeilo as given in the ' Silurian System.' But it is erroneous; as No. 1 ought, in a true vertical section, to be above No. 2. The origin of this mistake is explained by help of the profile section, fig. 6, which represents the general position of the groups near Llandeilo, without pretending to give any of the intricate contortions.

sections of North Wales.   Again, between No. 3 (the sandstone, &c. of Caradoc) and No. 4*a* (the May Hill series), there is a second break of continuity, to be supplied, perhaps, hereafter by a considerable series of intermediate strata.   With the conglomerates, &c. of No. 4*a*, commences a continuous series of deposits, representing the May Hill group and all the well-known Silurian beds above it.

We were unable, as before stated, to visit the Llandeilo sections; but I remember them sufficiently well to comprehend the cause of their original misinterpretation in the 'Silurian System.'

The section (fig. 5) represents the sequence of deposits in the Llandeilo country, as given by the author of the 'Silurian System;' and, for several years after the publication of that great work, it was accepted by geologists as a true and typical sequence.   Now, if the deposits represented by Nos. 2, 3, 4, &c. be unbroken and continuous, it will follow, of perfect necessity, that the neighbouring rocks of the Cambrian mountains (No. 1) must be inferior to them all; for, on this hypothesis, there is no place for their interpolation in any part of the section immediately below the Wenlock shale, &c. (No. 4).

In the hope of making this more clear, I give, in the accompanying ideal profile section (fig. 6), the general sequence and

Fig. 6.
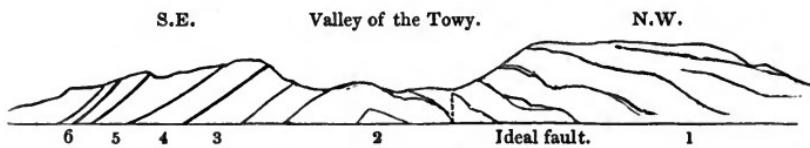
In this section No. 1 represents the commencement of the mountains which are coloured as Cambrian in the Silurian Map.

No. 2 is a part of the saddle of Llandeilo flag as seen in the Vale of the Towy.

Nos. 3, 4, 5, 6 agree with the succession in the vertical section, fig. 5.

position of the deposits in the neighbourhood of Llandeilo and Carmarthen.   In that typical country the Llandeilo flags exist as a broken and very contorted saddle in the position of the group (No. 2) in the Vale of the Towy.   On the south-eastern side of the contorted saddle, we find an *apparently* regular succession of deposits representing groups 3, 4, 5, &c. of the ideal vertical section (fig. 5); but on the other side of the valley of the Towy we have (towards the N.W.) a mountainous country, composed of slaty and often contorted rocks, which were considered in the Silurian Map as inferior to the Llandeilo flag, and to be represented by No. 1 in the ideal vertical section.

Now if the groups 2, 3, 4, 5, &c. be unbroken and continuous, we can only account for the position of the Cambrian group (No. 1) by the hypothesis of a great fault; and in this way, and this way only, can we bring the beds (No. 1) into coordination with their supposed place in the ideal vertical section (fig. 5).

As a matter of fact, however, there is no necessity for the intervention of a great fault, nor does any such fault exist*. The groups 2, 3, 4, &c. are *not* the parts of an *unbroken sequence*; but here (as in the vertical section, fig. 2) the Wenlock shales (No. 4) are, by a great overlap, brought into contact with the Llandeilo groups; and, to make the ideal vertical section (fig. 5) correct, we must remove the great Cambrian group (No. 1) from its place, and interpolate it below the Wenlock group (No. 4). In short, the relations of the older groups in the Llandeilo country have been completely misinterpreted in the 'Silurian System.'

It follows, from what has now been stated, that the sections, both of Caer Caradoc and Llandeilo, are broken and discontinuous sections, and that they cannot, therefore, be used correctly as typical sections for the establishment of a true geographical nomenclature. And I may here remark, that nearly all the mistakes of classification and controversies of nomenclature, respecting the older fossil-bearing groups of Wales, have arisen from an inattention to the fact, that all the upper groups, commencing with the May Hill sandstone, are, commonly, either unconformable to the Cambrian series, or, by some deceptive overlap, are brought into an abnormal superposition, which interferes with, and absolutely vitiates, their relations to the older beds on which they rest. It has constantly been assumed (sometimes very erroneously) that any shelly sandstone immediately under the Wenlock shale must be considered as a Caradoc sandstone; and this assumption inevitably led to another, viz. that a large group of rocks existed below the Wenlock shale (both in Cambria and Siluria), which contained characteristic groups of Cambrian and Silurian fossils inseparably united. From these two assumptions, followed a third—that all the fossiliferous rocks, from the highest Silurian to the lowest Cambrian, formed but one palæontological system.

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* It may be perhaps contended that the author of the 'Silurian System' does not, in his sections of the Towy, indicate the place of any great *fault*. But how are we to put the Llandeilo groups *over* the rocks which are coloured *Cambrian* without the supposition of a great upcast at the northern end of the sections? It is absolutely impossible to explain the sections of the Silurian System, pl. 34, fig. 9 (for example), without the intervention of a fault.

The establishment of the May Hill sandstone, as a true Silurian group, does away at once with all these three assumptions.

For several years after the publication of the 'Silurian System,' I believed that, between the lower groups of the Silurian system and the upper Cambrian groups, there was an overlap, or alternation of the beds. Hence I have sometimes described the rocks above the Bala limestone as " Cambro-Silurian ;" and, in 1843, " for the express purpose of avoiding any collision between the Lower Silurian groups and the Upper Cambrian," I proposed the name Protozoic for all the collective groups of Wales and Siluria which are below the Wenlock shale. At the same time I proposed the following general classification of the British palæozoic rocks. They were considered zoologically as *one system,* separable into four primary divisions, as follows :— " (1) Permian and Carboniferous ; (2) Devonian ; (3) Silurian, including (under that name) only the Upper Silurian rocks of Sir R. I. Murchison ; (4) Protozoic, as above defined." (Proceedings of the Geol. Society, vol. iv. June, 1843.) On this classification I may remark—1st, that I never used the word *Protozoic* as if it were synonymous with *Lower Silurian,* although this use of the word has been *very erroneously* attributed to me* ; 2ndly, that the establishment of the May Hill sandstone entirely does away with all ambiguity of nomenclature, or the necessity of employing any new terms, such as " Protozoic " or Cambro-Silurian ; 3rdly, that I have never, either before or since 1843, described the rocks under the Bala limestone by any other collective name than Cambrian.

In the vast sequence of the Cambrian deposits, which have a collective thickness of more than 30,000 feet, we find many indications of mechanical movements, without any great or sudden change of the organic types ; and it deserves remark, that some of the most violent of these movements, indicated by a succession of very coarse conglomerates, took place in South Wales, far from the immediate action of any plutonic rocks, among the very highest Cambrian groups ; and perhaps within the limits of those groups which may be arranged hereafter with the May Hill sandstone, and in that case must form a part of the Silurian or overlying series. Whatever be the true place of the *newest* of these coarse conglomerates (a point I had no opportunity of re-examining), it is obvious that about the end of the Cambrian

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* I here allude to an unwarrantable change made, *without my consent or knowledge,* in the nomenclature of my older Cambrian groups, during the passage of one of my papers through the press, and of which I was not allowed to see the proofs. See Proceedings of the Geol. Soc. vol. iv. p. 251, and the accompanying map, where I am represented as making *Lower Silurian* the equivalent of *Protozoic.*

deposits violent mechanical movements and shifting conditions, gradually produced an extinction of many of the older and most characteristic organic types. And in the discordant position of the overlying May Hill sandstone, we have not only the proof of great anterior mechanical movements among the upper Cambrian groups, but in it we find the commencement of a new succession of the regular Silurian deposit; and along with this new physical succession we find a co-ordinate change of the organic types. For in the May Hill sandstone the old characteristic organic types have suddenly disappeared; and in their place we have another set of types, which are characteristic of the lower groups of Siluria. In short (to use a language now in common use), we have in this way the introduction of a new "system," physically and palæontologically distinct from the whole system of Cambria.

Taking the May Hill sandstone as the base of the Silurian groups, I venture to affirm that no two subdivisions of the whole British palæozoic series are better defined, by physical characters, than are the collective Cambrian and Silurian groups. So far as regards the organic remains, I may remark that in the North of England, if all the fossil-bearing rocks below the Old Red Sandstone be divided into two groups, the upper group ending with the equivalent of the May Hill sandstone, out of about 180 well ascertained species of fossils, we do not find more than $3\frac{1}{2}$ per cent. of such species as common to both groups. The number of species common to the Cambrian and Silurian rocks, as they are developed in Wales and the bordering counties, is considerably greater; probably amounting to seven or eight per cent *. Much of the confusion between Cambrian and Silurian types has not been the work of Nature's development, but has been introduced by erroneous sections, and an erroneous grouping of the May Hill sandstone with the upper sandstones of the great Bala group. In conformity with the title of this paper, I next proceed to give a tabular view of the whole palæozoic system of England.

* A much larger per-centage of common species may be made out of the tables appended to the 2nd Fasciculus of the Cambridge Palæozoic Fossils. But when proper corrections have been made by the introduction of the May Hill sandstone as the true base of the Silurian series, and by the elimination of those localities which, in the field, give no sectional evidence of their true geological place, Professor M'Coy does not think that the common species will form a larger per-centage than that above given.

XLI. *Proceedings of Learned Societies.*

[Continued from p. 236.]

June 15, 1854.—The Earl of Rosse, President, in the Chair.

THE following papers were read :—

" On the Effect of the Pressure of the Atmosphere on the Mean Level of the Ocean." By Captain Sir James Clark Ross, R.N., F.R.S.

The author states that, in September 1848, Her Majesty's ships Enterprize and Investigator having anchored in the harbour of Port Leopold in lat. 74° N. and long. 91° W., a heavy pack of ice was driven down upon and completely closed the harbour's mouth, thus effectually preventing their egress, and compelling them there to pass the winter of 1848–49. It was during that period that the series of observations here presented to the Royal Society was obtained; and, as the observations were made under peculiarly favourable circumstances, the author considers they will throw some light on the movements of the tides, and on some of the causes of their apparent irregularities.

Soon after the harbour had been completely frozen over, a very heavy pressure from the main pack forced the newly-formed sheet of ice, which covered the bay, far up towards its head, carrying the ships with it into such shallow water that at low spring-tides their keels sometimes rested on the ground. Under these circumstances the movements of the tides became to the author an object of great anxiety, and consequently of careful observation, in order to ascertain the amount of irregularities to which they were liable in that particular locality.

The first few days' observations evidenced much larger differences in the elevation or depression of successive high or low-waters than could be accounted for by any of the generally received causes of disturbance; and the author was at once led to connect them with changes of the pressure of the atmosphere, from perceiving that on the days of great atmospheric pressure high-water was not so high as it ought to have been, and low-water was lower than its proper height; and that the reverse took place on the days of smaller pressure.

As it was found that the usual method of determining the mean level of the sea, by taking the mean of successive high- and low-waters, was inadequate to the detection of small quantities arising from a change in the pressure, a system of observation was adopted different from that heretofore practised, in order to determine the mean level of the sea on each day.

In the first instance, simultaneous observations of the height of the tide and of the mercury in the barometer were made every quarter of an hour throughout the twenty-four hours. From these it was found that the mean level of the sea for each day could be determined with great accuracy, and that the variation in the daily

mean level and in the mean pressure of the atmosphere followed each other in a remarkable manner, so that a rise in the former corresponded to a diminution in the latter. Subsequently however hourly observations were adopted.

The peculiar advantages of the position of the ships at Port Leopold for making tidal observations are stated to have consisted in :—

1. The great width of the entrance of the harbour admitting the free ingress and egress of the water, combined with the large field of ice which covered the whole of the bay, completely subduing every undulation of the water.

2. The steady movement of the immense platform of ice, rising and falling with such singular regularity and precision as to admit the reading off the marks of the tide-pole with the greatest exactness, even to the tenth of an inch.

3. The shallowness of the water and the evenness and solidity of the clay bottom admitting the fixture of the tide-pole with immoveable firmness.

4. The whole surface of the sea in the neighbourhood being, for the greater part of the time, covered by a sheet of ice, preventing those irregularities which occur in other localities from the violence of the wind raising or depressing the sea in as many different degrees as it varied in strength or duration.

For fixing the tide-pole for the "Enterprize" a hole 2 feet square was cut through the icy platform, and a strong pole, nearly 40 feet long, was passed through it and driven firmly down several feet into the clay, being fixed by heavy iron weights, which also rested on the clay and prevented any movement of the pole. It was placed in about 21 feet depth of water at the time of mean level of the sea. Another such tide-pole was, in a like manner, fixed through a hole in the ice close to the "Investigator," for the sake of reference and comparison.

Hourly observations of the height of the tide and of the barometer were commenced on the 1st of November, and were continued by the officers of each ship throughout the whole of the nine following months to the end of July. After forty-seven days of observation an interruption in one of the series occurred in consequence of the tide-pole of the "Enterprize" having been drawn up by the ice, to the under part of which it had become frozen. The amount of displacement of the pole was easily determined by a comparison with that of the "Investigator," but several days elapsed before it could be satisfactorily fixed at the same point in which it had been originally. The observations of these forty-seven days are those which are given in the paper, and their discussion is the immediate object of the communication.

It is stated that subsequent observations seem to show that, from the time of the interruption to the middle of July, there was a progressive elevation of the mean level of the sea, which, although of small amount, was sufficiently evident from month to month to render the subdivision of the series desirable, in order that the individual observations of each separate division should be strictly comparable.

The height of the sea and the corresponding height of the mercury in the barometer, at every hour in each day, from the 1st November to the 18th December 1848 are given in tables. In these the arithmetic mean of the hourly heights of the sea for each day is taken as the mean level of the sea for that day, and the mean of the hourly heights of the barometer is taken as the corresponding height of the barometer. These mean levels and corresponding mean barometric heights are given in another two-column table, arranged in the order of the days of observation; and in a third table these are arranged in the order of the heights of the barometer with the corresponding mean levels, without regard to the dates of observation, for the purpose of showing the dependence which the latter have on the former.

On these tables the author makes the following remarks. The forty-seven days of hourly observations give for the mean height of the barometer 29·874 inches, and of the mark of the mean level of the sea 21 feet 0·21 in.

The mean of three days } 30·227, and of corresponding level 20 feet 8·4 inch.
 greatest pressure was }

The mean of three days } 29·559, and of corresponding level 21 feet 5·4 inch.
 least pressure was ... }

Diff. +0·668   Diff. −9·0

Thus a difference of pressure equal to 0·668 inch produced a difference of 9 inches in the mean level of the sea. As the ratio of 9 to ·668 is 13·467 to 1, the author considers that the effect of the pressure of the atmosphere on the level of the sea is 13·467 times as great as the effect it produces on the mercury in the barometer, or very nearly in the inverse ratio of the specific gravities of sea-water and mercury. He however states that this remarkable coincidence must be considered in a great measure accidental, for if a greater number of days' observation be taken in order to deduce the mean greatest and mean least pressure, and the corresponding mean levels, a different result will be obtained. From these observations however he considers that he has been enabled to deduce results which plainly point to the law which governs the effect of the pressure of the atmosphere on the mean level of the sea, and may be encouraged to pursue the investigation through a more extended series of observations, in order to arrive at the most accurate conclusion that the observed facts may justify.

In conclusion a formula is given for determining the correct height of the tide, or of the mean level of the sea:—

Let L denote the correct height of the tide, or of the mean level of the sea;

 B the mean pressure of the atmosphere;

 λ the observed height of the tide, or of the mean level of the sea;

 β the corresponding height of the barometer;

 D the ratio of the specific gravity of mercury to that of sea-water:

then $L = \lambda + (\beta - B)D$.

Examples are given of the application of this formula.

"On the Thermal Effects of Fluids in Motion."—No. II. By J. P. Joule, Esq., F.R.S., and Professor W. Thomson, F.R.S.

The first experiments described in this paper show that the anomalies exhibited in the last table of experiments, in the paper preceding it*, are due to fluctuations of temperature in the issuing stream consequent on a change of the pressure with which the entering air is forced into the plug. It appears from these experiments, that when a considerable alteration is suddenly made in the pressure of the entering stream, the issuing stream experiences remarkable successions of augmentations and diminutions of temperature, which are sometimes perceptible for half an hour after the pressure of the entering stream has ceased to vary.

Several series of experiments are next described in which air is forced (by means of the large pump and other apparatus described in the first paper) through a plug of cotton wool, or unspun silk pressed together, at pressures varying in their excess above the atmospheric pressure, from five or six up to fifty or sixty pounds on the square inch. By these it appears that the cooling effect which the air, as found in the authors' previous experiments, always experiences in passing through the porous plug, varies proportionally to the excess of the pressure of the air on entering the plug above that with which it is allowed to escape. Seven series of experiments, in each of which the air entered the plug at a temperature of about 16° Cent., gave a mean cooling effect of about ·0175° Cent., per pound on the square inch, or ·27° Cent. per atmosphere, of difference of pressure. Experiments made at lower and at higher temperatures showed that the cooling effect is very sensibly less for high than for low temperatures, but have not yet led to sufficiently exact results at other temperatures than that stated (16° Cent.) to indicate the law according to which it varies with the temperature.

Experiments on carbonic acid at different temperatures are also described, which show that at about 16° Cent., this gas experiences $4\frac{1}{2}$ times as great a cooling effect as air. They agree well at all the different temperatures with a theoretical result derived according to the general dynamical theory from an empirical formula for the pressure of carbonic acid in terms of its temperature and density, which was kindly communicated by Mr. Rankine to the authors, having been investigated by him upon no other experimental data than those of Regnault on the expansion of the gas by heat and its compressibility.

Experiments were also made on hydrogen gas, which, although not such as to lead to accurate determinations, appeared to indicate very decidedly a cooling effect amounting to a small fraction, perhaps about $\frac{1}{16}$, of that which air would experience in the same circumstances.

The following theoretical deductions from these experiments are made :—

I. The relations between the heat generated and the work spent in compressing carbonic acid, air and hydrogen, are investigated from the experimental results. In each case the relation is nearly

* Philosophical Magazine for September 1853, p. 230.

that of equivalence, but the heat developed exceeds the equivalent of the work spent, by a very small amount for hydrogen, considerably more for air, and still more for carbonic acid. For slight compressions with the gases kept about the temperature 16°, this excess amounts to about $\frac{1}{77}$ of the whole heat emitted in the case of carbonic acid, and $\frac{1}{420}$ in the case of air.

II. It is shown by the general dynamical theory, that the air experiments, taken in connexion with Regnault's experimental results on the latent heat and pressure of saturated steam, make it certain that the density of saturated steam increases very much more with the pressure than according to Boyle's and Gay-Lussac's gaseous laws, and numbers are given expressing the theoretical densities of saturated steam at different temperatures, which it is desired should be verified by direct experiments.

III. Carnot's function in the "Theory of the Motive Power of Heat" is shown to be very nearly equal to the mechanical equivalent of the thermal unit divided by the temperature from the zero of the air-thermometer (that is, temperature Centigrade with a number equal to the reciprocal of the coefficient of expansion added), and corrections, depending on the amount of the observed cooling effects in the new air experiments, and the deviations from the gaseous laws of expansion and compression determined by Regnault, are applied to give a more precise evaluation.

IV. An absolute scale of temperature, that is, a scale not founded on reference to any particular thermometric substance or to any special qualities of any class of bodies, is founded on the following definition :—

If a physical system be subjected to cycles of perfectly reversible operations and be not allowed to take in or to emit heat except in localities, at two fixed temperatures, these temperatures are proportional to the whole quantities of heat taken in or emitted at them respectively during a complete cycle of the operations.

The principles upon which the unit or degree of temperature is to be chosen, so as to make the difference of temperatures on the absolute scale, agree with that on any other scale for a particular range of temperatures. If the difference of temperatures between the freezing and the boiling-points of water be made 100° on the new scale, the absolute temperature of the freezing-point is shown to be about 273°·7 ; and it is demonstrated that the temperatures from the freezing-point on the new scale will agree very closely with Centigrade temperature by the standard air-thermometer ; quite within the limits of the most accurate practical thermometry when the temperature is between 0° and 100° Cent., and very nearly if not quite within these limits for temperatures up to 300° Cent.

V. An empirical formula for the pressure of air in terms of its density, and its temperature on the absolute scale, is investigated, by using forms such as those first proposed and used by Mr. Rankine, and determining the constants so as to fulfill the conditions (1) of giving the observed cooling effects, (2) of agreeing with Regnault's observations on expansion by heat, and (3) of agreeing with Regnault's experimental results on compressibility at a particular temperature.

A table of comparison of temperature by the air-thermometer under varied conditions of temperature and pressure with the absolute scale, is deduced from this formula.

Expressions for the specific heats of any fluid in terms of the absolute temperature, the density, and the pressure, derived from the general dynamical theory, are worked out for the case of air according to the empirical formula; and tables of numerical results derived exclusively from these expressions and the ratio of the specific heats as determined by the theory of sound, are given. These tables show the mechanical values of the specific heats of air at different constant pressures, and at different constant densities. Taking 1390 as the mechanical equivalent of the thermal unit as determined by Mr. Joule's experiment on the friction of fluids, the authors find, as the mean specific heat of air under constant pressure,

·2390, from 0° to 100° Cent.
·2384, from 0° to 300° Cent.

XLII. *Intelligence and Miscellaneous Articles.*

NOTE ON THE PRODUCTION OF PYRO-ELECTRIC CURRENTS.
BY M. BECQUEREL.

THE disengagement of electricity is produced by various means— friction, heat, light, the action of magnets, induction, molecular and chemical actions, &c. I have endeavoured to produce the electric force by combining the action of heat with that of the affinities. My expectations have been realized; I have succeeded in producing currents which I shall call *pyro-electric currents,* by analogy with the currents obtained in the ordinary batteries, and to distinguish them from the thermo-electric currents, which are due to heat alone.

These currents, which have a constant power as long as the temperature does not vary very sensibly, are produced whenever solid metallic or other substances, conductors of electricity, are in contact with glass or any other vitreous substance in a state of igneous fusion, or softened by heat; but the greatest effect takes place only when the substance is fused.

In the memoir which I presented to the Academy on the 1st of May last, on the disengagement of electricity in chemical actions[*], I showed that glass even at a slightly elevated temperature began to conduct electrical currents, and that this property might be made use of to study the disengagement of electricity produced by the contact of platinum wires with flame. This conductibility begins to be sensible at about 482° F. I have since tried to ascertain whether, this conducting power increasing with the temperature, it would not be possible when the glass is fused, or even before this takes place, to substitute it for the acids and saline solutions in voltaic batteries. I operated in the following manner :—

First experiment.—If a rod of soft iron and a rod of copper be introduced into a furnace filled with lighted coals, each of them being in communication with the ends of the wire of an ordinary multiplier,

[*] Phil. Mag. for July 1854.

by means of an iron and a copper wire, the magnetic needle is not deviated, whatever may be the temperature. But it is very different if the copper rod be inclosed in a tube of hard glass, and the temperature be brought to the point of fusion of the latter. If a multiplier and a tangent galvanometer be placed in the circuit, it is seen that long before the glass has become red-hot, the needle of the multiplier deviates; by continuing the heat up to the melting-point, the intensity of the current constantly increases, attains a maximum, and remains constant. Long before this it is necessary to take away the multiplier and only make use of the tangent galvanometer; this current is directed from the iron to the copper, across the charcoal and glass; that is to say, the iron during its oxidation disengages negative electricity, and the copper, of which the surface remains clear and bright, evolves positive electricity. Thus the copper, although exposed to a high temperature, remains intact, as is also the case when, in contact with zinc or iron, it is immersed in an oxidizing liquid. It is consequently indebted to its electro-negative state for its preservation at an elevated temperature. The current remains constant as long as the temperature undergoes no sensible variation and the iron does not become covered with a thick layer of oxide; but should the tube partially fuse and the copper touch the iron, all the signs of electricity disappear. This fact proves that the current is not thermo-electric.

The disengagement of electricity, under these circumstances, is due therefore both to calorific and chemical causes. Whilst the iron is becoming oxidized it acquires a negative electricity, the surrounding air taking the positive electricity, which is transmitted to the copper by the medium of the red-hot coals and glass with which the gases are in contact.

Second experiment.—To ascertain the relation existing between the current produced by the pyro-electric couple and that arising from a Bunsen's couple of equal conductibility, I placed in the same circuit a pyro-electric couple, and another with nitric acid, of which the elements were of the same dimensions. These two couples were placed successively so that the currents passed in the same and in contrary directions.

Representing the intensity of the current furnished by the Bunsen's couple by x, and that of the pyro-electric current by y, it was found with the tangent magnetoscope that

$$x+y= \sin 17°=29237, \quad x-y= \sin 10°=17365 ;$$

whence $x=23301$, $y=5936$, and consequently $x : y : : 3·9 : 1$. Under the circumstances of my experiment the intensity of the pyro-electric current was therefore only a fourth of that of the nitric acid battery.

We have already seen that sufficient heat for the complete fusion of the glass must be avoided, as the iron and copper then soon touching, all the signs of electricity would gradually disappear; but there is another cause which diminishes the intensity of the current, the oxidation of the points of junction of the metallic wires and the rods of iron and copper, when they are very near the hot furnace; this inconvenience is got rid of by employing long rods,

which enable the points of junction to be removed to a distance from the furnace.

Third experiment.—By the substitution of a cylinder of charcoal prepared in the usual manner for the iron rod, connecting it by means of a platinum wire with the tangent magnetoscope, a current passing in the same direction as that furnished by the iron and copper, was produced. In this case then we have the current resulting from the combustion of the charcoal.

By comparing in the same manner as with the iron, the current produced by charcoal and copper with that of a nitric acid couple, the following results are obtained,—

$$x + y = \sin 14°·30 = 25038$$
$$x - y = \sin \ 8°·50 = 15357,$$

hence $x = 20197$, $y = 5340$, and consequently $x : y :: 3·76 : 1$.

Fourth experiment.—To compare these two sources of electricity exactly, it is necessary to determine, by means of Ohm's law, the conductive power of the sources themselves. The experiments which I have hitherto made show, that under the most favourable circumstances, when the temperature approaches the melting-point of copper, the conductive power of the two sources is nearly the same, but in proportion to the distance from this point the resistance gradually increases in the pyro-electric couple.

Fifth experiment.—The pyro-electric currents produce chemical decompositions like other currents.

With two plates of platinum and a Bunsen's couple, water is decomposed pretty rapidly, whilst with a pyro-electric couple acting at a temperature far below the melting-point of copper the current is distinctly stopped, at least if the positive platinum plate be not replaced by a plate of copper; in this case the disengagement of hydrogen gas is tolerably abundant. The same thing takes place when a solution of sulphate of copper is substituted for the acidulated water; with two plates of copper the sulphate is decomposed.

Observations.—The pyro-electric couples may be prepared in various ways, I shall mention three:—

1. Into an ordinary reverberatory furnace, an earthen crucible is introduced, lined internally with a thick plate of copper, turned so as to fit the interior of the crucible, and furnished with a wire of the same metal passing through an earthen tube to preserve it from oxidation. The crucible is filled with pounded glass, in sufficient quantity to cover the plate of copper, when fused, to a thickness of two centimeters. In contact with the glass, and resting vertically upon one end, is placed a bar of iron, long enough to pass through the top of the furnace; to the upper end is attached a wire of the same metal, which serves both to maintain it in its proper position and to connect it with the tangent magnetoscope or any other apparatus.

2. After filling the crucible with pounded glass to which 0·25 of carbonate of soda has been added to hasten the fusion, two long rods of iron and copper are introduced into it, and maintained in a vertical position, without contact, by means of iron and copper wires attached to their free extremities, which also serve as conductors. As soon as the glass is fused, the oxide of iron formed is dissolved,

and the surface of the iron rod always remains clean, so that the current remains constant. A heat sufficient for the fusion of the copper must be avoided. The current has a certain intensity long before the fusion of the glass.

3. A tube of green glass, enclosing a cylinder of copper, is put into a pistol barrel and all the interstices filled up with pounded glass; the whole is then placed horizontally in a proper furnace, and the pistol barrel and cylinder of copper connected with the other apparatus by means of wires of the same metals. This arrangement has given me the best results.

Sixth experiment.—In the pyro-electric couples just described, copper has been employed as the electro-negative element; but platinum and coke may also be used, although both of them present certain disadvantages. The platinum is attacked by the glass and disintegrated; the coke burns very slowly and produces a current in the opposite direction, which diminishes the action of the current resulting from the oxidation of the iron. It is possible, I think, to get rid of this inconvenience by introducing a cylinder of coke into an earthen tube and closing the openings with earth to prevent the circulation of air.

Observations.—Glass is not the only vitreous substance which may be employed; amongst those tried by me, I will mention borax, which, however, I gave up because it attacks the elements of the couple too rapidly. Common salt and nitrate of potash give but feeble actions, unless the latter salt be employed with coke,—a couple which gives a very powerful disengagement of electricity at the moment of deflagration of the coke; but from its rapid action and the danger attending it, this couple cannot be made use of.

Sand and pure quartz, whatever temperature they may be exposed to, never acquire the conductive power, and cannot be substituted for glass or the alkaline silicates.

The facts described in this note, show that the lost heat of factories may be employed to set in action pyro-electric couples, producing currents which partake of the nature of hydro-electric and thermo-electric currents. They also render it probable that terrestrial-electric currents exist, at the point of contact of the solid part of the globe with the fused portion, where solid conducting substances are partially imbedded in fused silicates, in the same way as in pyro-electric couples.—*Comptes Rendus,* May 22, 1854, p. 902.

ON HYDROCYANALDINE. BY A. STRECKER.

Some years ago I found that a mixture of aldehydate of ammonia and hydrocyanic acid, with an excess of muriatic acid, when evaporated on the water-bath, gives a residue of muriate of ammonia and muriate of alanine,—

$$C^4 H^4 O^2 NH^3 + 2HCl + C^2 NH + 2HO = C^6 H^7 NO^4 . HCl + NH^4 Cl.$$
Aldehydate of   Muriate of alanine.
ammonia.
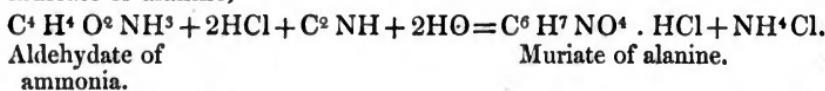
The reaction is quite different when the same mixture is not heated. In this case colourless crystals are formed in the fluid in the course of a few days, and these increase by degrees. It is to

this body that I give the name of *hydrocyanaldine.* It is insipid, has no reaction upon vegetable colours, dissolves in water, æther, and more readily in alcohol. It fuses at a low temperature, and sublimes at a moderate heat; when briskly heated it is decomposed with an odour analogous to that of hydrocyanic acid. The solution of this body is not precipitated by salts of silver, even after the addition of nitric acid; but when the solution is heated with silver salts, cyanide of silver is precipitated and aldehyde developed. When heated with potash, hydrocyanaldine evolves ammonia, and the solution becomes brown, with separation of resin of aldehyde.

The analysis of hydrocyanaldine gave me the formula $C^9 H^6 N^2$ or $C^{18} H^{12} N^4$, and its formation is expressed by the equation—

$$3(C^4 H^4 O^2 NH^3) + 3C^2 HN + 2HCl = C^{18} H^{12} N^4 + 2NH^4 Cl + 6HO.$$
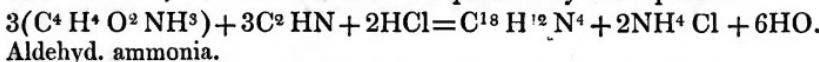
Aldehyd. ammonia.

This exhibits some analogy with the formation of thialdine,—

$$3(C^4 H^4 O^2 NH^3) + 6HS = C^{12} H^{13} NS^4 + 2NH^4S + 6HO.$$
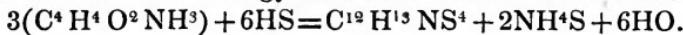
Hydrocyanaldine nevertheless differs greatly from thialdine, especially because it has not distinct basic properties; at least I have not succeeded in preparing saline compounds of this body.—*Comptes Rendus,* July 3, 1854, p. 55.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1854.

Chiswick.—August 1. Cloudy: clear. 2. Clear: very fine: heavy rain at night. 3. Overcast: rain. 4. Overcast: heavy rain. 5. Rain: overcast. 6, 7. Overcast: clear. 8. Very fine. 9. Fine: overcast: very clear at night. 10, 11. Very fine. 12. Densely clouded: very clear at night. 13. Very fine. 14. Cloudy: very fine: clear. 15. Very clear: cloudy: clear. 16. Fine: overcast. 17. Fine: rain: very clear. 18. Fine: cloudy. 19. Very clear. 20. Cloudy and fine. 21. Cloudy. 22. Cloudy and fine: clear. 23. Overcast: rather boisterous, with rain. 24. Fine: cloudy and boisterous: fine. 25. Fine. 26. Exceedingly fine. 27. Very fine: overcast. 28. Very fine. 29. Very hot. 30. Cloudless and very hot, with dry air. 31. Very fine.

Mean temperature of the month .... 60°·70
Mean temperature of Aug. 1853 .... 59 ·69
Mean temperature of Aug. for the last twenty-eight years... 62 ·03
Average amount of rain in Aug. .... 2·47 inches.

Boston.—Aug. 1, 2. Cloudy: rain A.M. and P.M. 3. Cloudy. 4. Cloudy: rain A.M. and P.M. 5—9. Cloudy. 10, 11. Fine. 12, 13. Cloudy. 14. Cloudy: rain P.M. 15. Fine: rain P.M. 16. Fine. 17. Cloudy: rain P.M. 18. Cloudy. 19. Fine. 20. Fine: rain. 21. Cloudy: rain A.M. 22. Fine: rain, with thunder and lightning P.M. 23. Cloudy. 24. Fine: rain P.M. 25. Fine. 26, 27. Cloudy. 28—30. Fine. 31. Cloudy.

Sandwick Manse, Orkney.—Aug. 1. Fog A.M.: drizzle P.M. 2. Bright A.M.: clear P.M. 3, 4. Cloudy A.M. and P.M. 5. Damp A.M.: drizzle P.M. 6. Cloudy A.M.: drizzle P.M. 7. Cloudy A.M.: damp P.M. 8, 9. Cloudy A.M.: drops P.M. 10. Cloudy A.M.: clear, fine P.M. 11. Bright A.M.: bright, fine P.M. 12. Cloudy A.M.: showers P.M. 13. Showers A.M.: clear, fine P.M. 14. Cloudy A.M.: rain, thunder P.M. 15. Bright A.M.: cloudy P.M. 16. Bright A.M.: clear P.M. 17. Bright, fine A.M.: clear, fine P.M. 18. Clear, fine A.M.: cloudy P.M. 19. Cloudy A.M.: showers, aurora P.M. 20. Bright A.M.: cloudy, fine P.M. 21. Drops A.M.: showers P.M. 22. Showers A.M.: cloudy P.M. 23. Rain, cloudy A.M.: showers P.M. 24. Showers A.M. and P.M. 25. Cloudy A.M. and P.M. 26. Drizzle A.M.: cloudy P.M. 27. Damp A.M.: cloudy P.M. 28. Cloudy A.M. and P.M. 29. Cloudy A.M.: rain P.M. 30. Showers A.M. and P.M. 31. Showers A.M.: cloudy P.M.

Mean temperature of Aug. for twenty-seven previous years . 54°·99
Mean temperature of this month .... 55 ·06
Mean temperature of Aug. 1853 .... 55 ·98
Average quantity of rain in Aug. for fourteen previous years . 2·99 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, *near London; by Mr. Veall, at* BOSTON; *and by the Rev. C. Clouston, at Sandwick Manse,* ORKNEY.

Days of Month. 1854. Aug.	Bar. Chiswick Max.	Bar. Chiswick Min.	Bar. Boston 8½ a.m.	Bar. Orkney Sandwick 9¼ a.m.	Bar. Orkney Sandwick 8¼ p.m.	Therm. Chiswick Max.	Therm. Chiswick Min.	Therm. Boston 8½ a.m.	Therm. Orkney 9¼ a.m.	Therm. Orkney 8¼ p.m.	Wind Chiswick 1 p.m.	Wind Boston	Wind Orkney Sandwick	Rain Chiswick	Rain Boston	Rain Orkney Sandwick
☽ 1.	29.697	29.658	29.914	29.973	29.977	69	46	63	58	55	sw.	wsw.	n.	.24	.44	
2.	29.783	29.684	29.918	29.981	29.988	73	56	61	56	51	nw.	w.	n.	.40	.11	
3.	29.854	29.805	29.938	29.997	30.006	58	46	61	53	52	ne.	nnw.	nnw.	.27	.39	
4.	29.929	29.916	29.945	30.008	30.011	57	52	51	53	52	n.	nnw.	nnw.	.39	.12	
5.	30.038	29.974	29.955	30.013	30.011	59	54	56.5	54½	52	ne.	nnw.	nnw.	.04	.24	
6.	30.110	30.060	29.965	30.006	30.000	67	49	60	57½	55	n.	nnw.	w.			
7.	30.059	30.037	29.963	29.991	29.981	68	44	61	60½	56½	sw.	ssw.	ssw.			
○ 8.	30.057	29.971	29.957	29.981	29.981	74	48	67	58½	57	sw.	sw.				
9.	29.944	29.882	29.946	29.981	29.977	77	48	66	58½	56	nw.	w.				.10
10.	29.838	29.791	29.937	29.976	29.977	77	48	63	55	54	w.	w.	ese.			
11.	29.947	29.934	29.944	29.980	29.978	75	58	65	56	54	w.	nw.	sw.			
12.	29.924	29.897	29.936	29.952	29.938	74	58	64.5	58½	56½	w.	sw.	sw.			.14
13.	29.902	29.731	29.938	29.969	29.981	86	54	70	56½	55	s.	w.	e.			.09
14.	29.836	29.726	29.925	29.968	29.968	72	52	65	57	52	sw.	w.	nnw.			
☾ 15.	29.845	29.836	29.933	29.971	29.976	71	43	62	54½	52	w.	sw.	w.	.01		
16.	29.954	29.836	29.940	29.976	29.984	67	42	53.5	55½	50	w.	w.	w.		.12	
17.	30.038	29.987	29.950	29.992	30.001	64	40	58	54	56	w.	w.	ssw.		.12	
18.	30.128	30.095	29.966	29.999	29.996	73	46	58	56	56	sw.	nw.	w.	.11		.34
19.	30.139	30.042	29.963	29.984	29.976	76	58	62	58½	56	sw.	sw.	ssw.	.10		
20.	29.996	29.924	29.947	29.974	29.976	75	49	66	57½	55	s.	sw.	w.		.04	
21.	29.824	29.765	29.932	29.956	29.931	70	50	68	55½	55	w.	w.	w.	.12		
22.	30.013	29.865	29.934	29.945	29.972	71	41	58.5	54½	54½	sw.	wnw.	w.	.01		
● 23.	30.108	30.000	29.960	29.966	29.937	67	57	59	56	54	sw.	wsw.	sw.		.03	.06
24.	29.996	29.935	29.937	29.917	29.962	72	47	62	56	51	sw.	sw.	nw.	.08	.14	.05
25.	30.266	30.158	29.964	30.005	30.019	70	39	58	51	52	nw.	w.	w.		.03	.06
26.	30.361	30.330	29.987	30.016	30.011	76	51	60	56½	58	n.	calm	wsw.			.28
27.	30.398	30.395	29.987	30.022	30.026	79	63	69	56½	59	nw.	calm	wsw.			.48
28.	30.438	30.425	29.992	30.017	30.027	82	50	67	60	54½	n.	nnw.	se.			.35
29.	30.432	30.319	29.990	30.027	30.003	84	45	69	54½	54	w.	calm	w.			.20
30.	30.252	30.127	29.970	29.984	29.982	84	54	62	56	53½	w.	w.	w.			.10
☽ 31.	30.260	30.157	29.972	30.001	30.016	73	40	61	53	53½	ne.	nw.	w.			.10
Mean.	30.044	29.976	29.951	29.856	29.871	72.45	48.96	62.0	56.09	54.04				1.77	1.78	2.40

XLIII. *Observations on Meteorolites or Aërolites, considered Geographically, Statistically, and Cosmically, accompanied by a complete Catalogue. By* R. P. GREG[*].

IT is many years since any attempt has been made to give a complete list of well-authenticated meteoric falls; recently, indeed, M. Partsch of Vienna has published an interesting account, as well as catalogue, of the meteoric irons and stones in the Imperial Museum of that city; and Professor Shepard of the United States has also given us a list of the meteorites in his own collection, as well as a *thesis* on American meteorites; but I am ignorant of anything approaching a complete or compendious catalogue of the falls of these bodies.

The accompanying catalogue has been carefully compiled from various sources[†]; where possible, concise particulars, not only as to date and locality are given, but mention is also made of weights, specific gravity, appearances, &c.; and several analytical and statistical tables are added, which may not be without importance in the present as well as future consideration of this subject.

Great care has been taken to avoid erroneous dates or confusion of localities; and queries are occasionally annexed, where there wants evidence to establish fully the authenticity or correctness of the fall.

[*] Communicated by the Author.

[†] Such as old volumes of the Philosophical Transactions; the Philosophical Magazine; Brewster's Encyclopædia, article "Meteorite"; Partsch's, Shepard's and Chladni's Catalogues; the volumes of the British Association; Silliman's Journal; *Comptes Rendus*; *Annales de Chimie et de Physique,* vol. xxxi.; Nicholson's Journal of Philosophy; Professor Clark's Thesis on Iron Meteoric Masses; and sundry other periodicals, both scientific and literary.

It is more especially my present object to investigate some of the results apparently indicated by these tables, constructed purposely from the general catalogue; and I shall consider the subject, first *geographically, i. e.* with regard to the *geographical* distribution or deposition of aërolites on the surface of the globe; secondly, *statistically,* with reference to dates and numbers; and thirdly, if I may use the term, *cosmically.*

Considerable allowance must be made in the following, as indeed in all considerations respecting these singular bodies; but I am of opinion that the number of falls now brought together in a tabulated form will be sufficient to furnish us with some evidence, if indeed only of a negative kind, to start from. The three following tables tend to prove the not otherwise than pretty equable occurrence of meteoric falls on the surface of our earth, a point by no means without importance. Due allowance must of course be made for various counteracting influences, such as preponderance of sea and uninhabited countries in certain latitudes, and want of historical or scientific records among particular nations, &c.

Table A.

Countries.	Stones.	Irons.	Total.	Average latitude.
France	34	1	35	48 N.
Ireland and Great Britain	20	1	21	53 N.
Bavaria, Prussia; Germany	37	6	43	51 N.
Hungary, Bohemia; Austria	28	5	33	48 N.
Switzerland	2	......	2	46 N.
Lombardy, Piedmont, Sicily; Italy.........	33	1	34	43 N.
Portugal and Spain	7	......	7	40 N.
European Russia	14	1	15	54 N.
Finland and Siberia	4	3	7	63 N.
Sweden	1	......	1	60 N.
Asia Minor, Crete; Turkey.................	10	1	11	40 N.
Egypt, Arabia and N. Africa	6	1	7	30 N.
Tartary, Persia and Central Asia............	1	2	3	35 N.
Japan and China	23	......	23	18 N.
Ceylon and India	19	3	22	20 N.
United States	18	34	52	35 N.
Greenland	1	1	2	65 N.
West Indies and Mexico	2	8	10	25 N.
Sandwich Islands	1	......	1	20 N.
South Africa	2	2	4	30 S.
Java	1	......	1	10 S.
South America	1	5	6	20 S.
Totals	264	74	338	

Table B.—Showing the number of Meteoric Depositions recorded, arranged according to zones of Latitude, North.

Between N. latitude	5	and	10	....	3	
...	...	10	...	20	....	16
...	...	20	...	30	....	34
...	...	30	...	40	....	72
...	...	40	...	50	....	127
...	...	50	...	60	....	67
...	...	60	...	70	....	8
						327

Table C.—Showing the proportion of falls, for several countries, that might be supposed to occur, making due allowance for the relative extent and population of each, taking France as the standard or unit of comparison, and commencing with the year 1790.

	Actual number.	Computed number.
France...	19	19
Great Britain and Ireland....................	11	12
Spain	3	9
Germany...	11	13
Austria	14	13
Italy	11	14
European Russia	12 } 30	31 } 39
United States....................................	18	8

The number of meteoric falls observed for Great Britain, France, Germany, Austria and Italy, is thus shown to have been sixty-six, in a period of sixty-four years; taking the area of these five countries at 900,000 square miles, and that of the earth's surface at 197 millions, we obtain 219 as the number of annual falls likely, in the ordinary course of events, to be observed, were the whole surface of our globe peopled with an European density of population and a similar degree of civilization.

Taking, however, into consideration that one-half of mankind is alternately experiencing the darkness of night, when they are not so likely to observe the descent of these bodies or mark the exact spot where they reach the earth's surface, we may fairly, instead of 219, assume 400 as more nearly the number of falls likely to occur under the above-named conditions. What proportion 400 may bear to the entire number that fall, it is not easy to conjecture, though after mature consideration, I am inclined to think that number will *exceed* one-third of the whole. It is desirable to bear in mind the probability of a not *unequal* distribution of meteorite falls on the surface of the earth, because it

might appear from a too superficial or limited examination, that such was not the case, a view, indeed, apparently adopted by Professor Shepard, in some remarks he published in 1850, respecting the " Geographical Distribution " of these bodies. He considers that there are some regions of the earth's surface, or certain zones, towards or in which there is a tendency to " concentration in the deposition " of meteoric matter; and he instances particular countries, as Canada, Portugal, Spain, South Italy, Sicily, Hungary, Denmark, Sweden, Norway, and Northern Russia, which furnish few or no instances of meteoric deposition. As regards Canada and Norway only, can his remarks, I consider, strictly hold good, as will be admitted on a perusal of the localities given in the catalogue accompanying this paper: that there are some irregularities no one will deny, yet considering the strange nature of, and the phænomena exhibited by, these bodies, and making due allowance for various causes likely to affect an observable uniformity of deposition, it is only remarkable how uniformly they have everywhere been observed*.

Professor Shepard correctly takes for the United States the parallel of 37° N. as the line of greatest average meteoric deposition, and for Europe that of 46° N.

A line drawn through the centre of greatest meteoric deposition in America would, if prolonged so as to include the like centre for Europe, form, with the ordinary parallels of latitude, an angle of about 10° or 11°.

I shall now quote Prof. Shepard's own words :—

" If then it appears that these aërial strangers alight upon our earth in such great preponderance over limited areas, can we help admitting that there presides over their descent some great law, or in other words, that these falls take place in accordance with some fixed plan. The present stage of our knowledge may, indeed, be inadequate to develope what that plan actually is; but when we see so marked an approach by the courses of our meteoric regions, to the isothermal parallels for the same zones, and again, an observable coincidence between the trends of the meteoric regions, and the isodynamic lines, we are strongly tempted to refer the forces of greatest activity concerned in the phænomenon, to an union of thermal and magnetic action, although it is, at the same time, possible that more powerful local attractions in the surfaces concerned, than exist elsewhere, may also exert some influences over the deposition of these singular bodies."

I need not say more respecting this part of the subject, except

* For mention of some less important, though not less curious, irregularities concerning the fall and nature of meteorites, see Note I. at the end of this paper.

that I must differ from Prof. Shepard, and I have given my facts and reasons for so doing.

It would indeed be strange should these bodies,—varying in size and weight from half an ounce to 30,000 lbs., sometimes containing no iron at all, and occasionally composed of nothing but iron, with an oblique direction generally from east to west, and a velocity of fifteen to thirty miles in a second,—be attracted by particular countries more than others, or arrange themselves in zones parallel to the isothermal or isodynamic lines.

The next point I shall draw attention to, are the variations in the number of falls taken in five-yearly periods, from 1795 up to 1854 :—

From 1795 to 1800 are described... 7	From 1825 to 1830 are described... 11
... 1800 to 1805 6	... 1830 to 1835 7
... 1805 to 1810 13	... 1835 to 1840 12
... 1810 to 1815 15	... 1840 to 1845 13
... 1815 to 1820 9	... 1845 to 1850 10
... 1820 to 1825 12	... 1850 to 1854 6
Falls 62	Falls 59
	62
	Total............... 121

This gives an average of 10·9 for each of the twelve quinquennial periods, or nearly *two* per annum ; more are recorded for the first moiety of the sixty years than for the second, though one might have expected rather a marked increase during the second period, owing to the increase which has taken place during the last quarter of a century in population and intelligence, as well as facilities for procuring and disseminating information.

Indeed, as but one fall is recorded for each of the years 1851, 1852, 1853 and 1854, and but two for each of the years 1847, 1848, 1849 and 1850, while some years present us with three, four and even five instances of falls, one is almost led to imagine a temporary if not absolute falling off in the frequency of these phænomena; whether this may be owing to accident and chance, or to the existence of some unknown cause or cycle, we must, from want of more data, at present remain ignorant.

The following table, presenting an analysis of the total number of known falls I have been enabled to collect or hear of, arranged according to the falls for each month, from the year A.D. 1500 to 1854, shows some novel if not indeed important results.

Table D.

Month.	No.	Month.	No.
January	9	July	19¼
February..........................	15	August	15
March	17	September	17
April	14½	October	14
May	15	November	15
June	17	December	9
First half-yearly total	87½	Second half-yearly total ...	89¼

N.B. Average .... 14·75

It is rather singular how equal the number is for each half-yearly period, but the most important thing to notice is the great falling off for the months of December and January, and the almost corresponding increase for June and July; the two former together only show 18, while the two latter 36½, or more than double*.

It may be argued, this is in consequence of the days being longer in summer than in winter. While, however, there is but 16 per cent. more daylight in November than in December, the falls of meteorites are, it is seen, more than 50 per cent. more, and while there are nine falls recorded in January, there are fifteen in February, and seventeen in March, months when the days are still nearly as short. November shows considerably more also than December. The difference existing between different countries, in latitude and longitude, will also tend rather to equalize the difference that occurs in the duration or simultaneous commencement of night at any particular period of the year. The nine falls for January are spread over, be it observed, a very long period. There appear only to be four instances in the last hundred years.

There is doubtless then some other and more important reason required to account for this marked decrease in the number of aërolites observed in December and January, as well perhaps as for the larger number of falls which have occurred in June and July. (See Note II.)

Let it be borne in mind that the earth in her orbit at those periods of the year, is on the sides of the winter and summer solstices respectively, *i. e.* in *perihelion* and *aphelion*.

I shall revert to this part of the subject, and now proceed to the consideration of the following table which I have constructed, rather roughly indeed, from the reports of Professor Powell,

* Monsieur Marcel de Serres, in the *Annales de Chimie et de Physique,* vol. lxxxv. p. 262, remarks, that out of sixty-five falls, two-thirds were in June, July and August.

drawn up for, and published by the British Association, in the volumes of its Transactions for the years 1848 to 1853. At best these results can only be relative and approximative.

Column A. denotes the total number of luminous meteors described (or recorded and particularized) in the above-named reports; and column B. the number only of the most remarkable ones*.

Table E.—Luminous Meteors.

Months.	A.	B.	Per-centage of large ones.	Months.	A.	B.	Per-centage of large ones.
January	190	13	6·8	July	364	20	5·5
February.............	102	18	18·0	August	4370	25	0·6
March................	117	7	6·0	September	315	25	7·9
April	236	15	6·7	October	320	12	3·9
May	41	8	20·0	November	1470	24	1·7
June	88	12	13·6	December	310	19	6·1

On comparing this table with Table D., one is struck with several comparative dissimilarities of result. The marked poverty of meteors observed in March and May does not agree with the number of aërolites observed for the same months, as given in Table D., where March has over the average and May about the average number, for the whole year.

In Table E., December has nearly as many as July, September and October; and more than January, February, March, April, May and June. This may perhaps be the result of chance, but not so when we take the months of August and November (Table E.); these two months show a decided and even enormous preponderance in the number of luminous meteors observed, owing principally to the periodic displays which usually take place from the 9th to the 13th of each month.

Referring now to Table D., it will be observed that the number of meteoric stones or aërolites ascertained to have fallen for these two months, barely exceeds the average of the whole twelve months.

This deserves some attention, since out of more than 150 meteorites (or aërolites) whose precise date of fall are well ascertained, there are but *four* (see Table F.) which fell on any of the twelve days included between the 9th to 14th days of August and November respectively. From this we are, I think, justified in drawing the conclusion, that, with many phænomena in common, there does exist a distinction between meteoric stones or *aërolites* and *luminous meteors*. This distinction one may sup-

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* Such as those having a larger *apparent* size than the planet Jupiter, those accompanied by audible explosion, or such as are described as having approached particularly near the surface of the earth.

pose to be somewhat of the same character as that existing be-
tween planet and comet; the *former* composed of matter in a
solid form and revolving round the sun in orbits less elliptical
than the *latter*, but more so than those of the larger planets, the
latter having also a gaseous or perhaps fluid nature.

Some attempts have been made to ascertain the orbits of the
periodically recurring meteor showers of August and September,
and Professor Olmstead calculated that the one seen on the night
of the 13th of November 1833, had its *aphelion* near the earth's
orbit, and its *perihelion* within the orbit of Mercury; that is, its
mean distance from the sun lies *within* the earth's orbit.   (See
Note III.)

An examination of Table F. will, I am persuaded, convince
most people that there are *periodic* epochs for aërolites as well
as for luminous meteors.   This, if true, as I feel convinced
the sequel of future observations will prove to be the case, is
a new and important step gained towards a just consideration
of these bodies, and determines for them a place in our solar
system.

I have just expressed the opinion that there probably existed
a difference between aërolites and luminous meteors or meteoric
showers, and based that opinion partly on the fact that there is
no increase in the number of aërolites or meteoric stones which
have fallen to the earth at those periods *most* remarkable as
epochs for luminous meteors; and on comparing the aërolite
epochs (see notes *a* and *b*, Table F.) with those for luminous
meteors, this opinion is still further borne out.   There is, how-
ever, distinctly one exception, and that is November 27–29,
an epoch apparently common to both classes alike.   It remains,
however, to be seen if their periods as well as epochs agree, and
whether those years in which the aërolites fall are also unusually
rich in meteors.   (See Note IV.)

Connecting as I do aërolites with the system of the asteroids,
and allowing that the earth, at the period of *aphelion* or greatest
distance from the sun, may be most liable to come near or in
contact with them, we must also consider them, like the aste-
roids, as having a greater mean distance than the earth from
the sun, *i. e.* as lying principally *without* the earth's orbit.

I am not now proposing any new theory, but only supporting
and carrying out the supposition long ago entertained by
Dr. Chladni, and since then advocated by most astronomers, that
meteoric stones are *true*, though minute, planetary fragments;
but from want of data, no serious attempt at anything amount-
ing to demonstrative proof has yet been made; and there are
now many scientific men who attribute to them an atmospheric
or lunar origin.   When, therefore, there is still so much conjec-

ture and so much confusion respecting the nature, origin and phænomena of these bodies, any ray of light is acceptable to the theorist, and anything like fact or tabulated statistics, of value. In continuation, then, and in search of further evidence, this part of our subject may also be incidentally considered in another way.

The average specific gravity of about seventy stones[*] I find to be 3·4, the highest being about 3·95 and the lowest 1·7; but as those possessing the smallest specific gravity are necessarily the most destructible and fragile, and after meteoric explosion less likely to arrive on the surface of the earth in an entire or tangible state, we may very fairly take their average density nearer the mean of these two extremes, say 3·0.

We may now construct a table of densities (taking water as 1), which is not without interest, as perhaps bearing on the subject in hand.

The specific gravity of Mercury is about	. .	15·7
... Venus ...	. .	5·9
... Earth ...	. .	5·7
... Mars ...	. .	5·3
... Aërolites (3·9 to 1·7, say)		3·0
... Asteroids	. .	(?)
... Jupiter ...	. .	1·4

This then looks like additional confirmation of the theory of what are called meteorites, or aërolites, belonging to the series of planets, and having their orbits at a greater mean distance than that of the earth's from the sun. Bearing this in mind, as also the probability of the fact of our meeting with more of them on the side of the summer solstice, or when the earth is at her *aphelion,* I would draw attention to the following extract from a paper in the American Journal of Science for July 1854, entitled "Considerations on the group of small Planets situated between Mars and Jupiter," by M. U. J. Le Verrier; the paper in question being a translation and abbreviation of the original in the *Comptes Rendus,* vol. xxxvii. p. 793 :—

"If the perihelia of the asteroids, known and unknown, were distributed uniformly in all parts of the zodiac, the second term of the motion of the perihelion of Mars or of the earth might be neglected; because the action of those masses, whose perihelia are situated in one-half of the heavens, would be destroyed in this second term by the action of those masses whose perihelia are in the other half. But we have seen that there is great liability to error in reckoning upon such a uniformity in their distribution; the perihelia of twenty out of twenty-six being placed in

[*] Iron falls are comparatively very rare, as compared with stone falls; the usual specific gravity of meteoric stones is about 3·5.

one-half of the heavens, a result doubtless not of chance, and seeming to indicate that the matter *whose mass we are investigating is nearer the sun on the side of the summer solstice than of the winter.* This circumstance must be taken into consideration, not for the purpose of introducing it as an essential condition into the solution of the problem, but, on the contrary, of arriving at a result which shall be independent of it.

" This consideration will lead us not to make use of the motion of the earth's perihelion, although it is better known than that of Mars. The earth's perihelion being in fact situated in that very portion of the heavens occupied by the perihelia of more than three-fourths of the asteroids, the second term which enters into the expression of its motion may become appreciable as compared with the first and of the contrary sign : inasmuch as these terms are respectively proportional to the excentricities of the terrestrial orbit and the orbits of the small planets, and as the excentricities of these last are at the mean *nine* times greater than that of the earth.

" The perihelion of Mars is situated much more favourably in relation to the mean direction of the perihelia of the asteroids ; and besides the excentricity of its orbit is greater. As a result of these two conditions united, the second term which enters into the expression of the motion of the perihelion is only *one-fourth* of the first. Now this superiority of the first term may be expected to continue after the discovery of a great number of new asteroids, *whether this predominance of the perihelia in the mean direction of the summer solstice shall be confirmed, as it probably will be,* or whether we shall be obliged to return to the idea of a uniform distribution of them through every part of the heavens.

" In accordance with these remarks, I have found that if the mass of the whole group of asteroids was equal to the mass of the earth, it would produce in the heliocentric longitude of the perihelion of Mars an inequality which in a century will amount to eleven seconds. Such an inequality, supposing it to exist, surely could not have escaped the notice of astronomers. If we reflect that this inequality will become strikingly sensible at the moment of the opposition of Mars, we must believe that at present, and although the orbit of Mars has not been determined with perfect accuracy, it cannot nevertheless admit of an error in longitude greater than one-fourth of the inequality which we have pointed out. Hence we conclude that *the sum total of the matter constituting the small planets situated between the mean distances* 2·20 *and* 3·16 *cannot exceed about one-fourth of the mass of the earth.*"

In a second memoir (*Compt. Rendus,* t. xxxvii. p. 965) M. Le Verrier establishes the following propositions :—

1. " The excentricities of the orbits of the known asteroids can suffer very small changes as the effect of perturbation. These excentricities, which are now quite large, have then always been and will always remain large.

2. " The same is true of the inclination of their orbits; so that the amount of excentricity and inclination answers to the primitive conditions of the formation of the group.

3. " These propositions are only true for distances from the sun above 2·00. An asteroid situated between Mars and the distance of about 2·00 would not be *stable* in the meaning which is attached to that word in celestial mechanics.

" *Flora,* which is nearest to the sun of the known asteroids, is 2·20 distant. M. Le Verrier also observes that it is remarkable that a planet has been found almost up to the line which theory assigns as the limit of stability, and that none have been found beyond it. Must we believe that the same cause which has given origin to so many asteroids above the distance 2·00, *has also distributed them below this distance*? but that the excentricities and inclinations of these last being considerably increased, it is at present difficult to discover them, especially because towards their perihelion they will be immersed in the light of the sun, and that coming to their opposition only in their aphelia, they then will be too far from us?

4. " Owing to the magnitude of their excentricities and their inclinations and the smallness of their variations, the mean motions of the perihelia and of the nodes are proportional to the times."

From the above extracts, it would appear, according to Le Verrier, that there is a probable predominance of the perihelia of the asteroids in the mean direction of our summer solstice; a circumstance, if true, quite confirmatory of the opinion I have expressed, that the increase observable in the number of falls recorded for the months of June and July is not quite the result of chance. What Le Verrier says also respecting the probability of undiscovered asteroids outlying as it were the mean limit of stability, argued inductively, is also interesting, and indirectly bears on the point we have been considering. The extreme degree of ellipticity assigned also to the orbits of the asteroids should be noticed.

Proceeding still further in our investigations of this part of the subject, I shall beg leave to make the following quotation from " Smyth's Celestial Cycle," p. 159. vol. i., on the subject of the asteroids.

" Borrowing from La Place's conjecture before alluded to of a great contraction of the sun's atmosphere, a convulsive disorganization of some planet may be supposed to have taken place,

by a force capable of overcoming the mutual attraction of its particles, and the mass of matter so broken would inevitably be dispersed in every direction, and in parts of various sizes.

"The impulses given by the explosion would gradually diminish, and the parts, in gravitating towards the sun, would become influenced by progression and rotation. To this view there does not appear to be any demonstrable objection. It was suggested that under such a disruption the form of the orbits assumed by the fragments, and their inclination to the ecliptic, or to the orbit of the original planet, would depend upon the size of the fragments, or the weight of their respective masses : *the larger mass would deviate least from the original path, while the smaller fragments being thrown off with greater velocity, will revolve in orbits more excentric and more inclined to the ecliptic.* Now that is precisely what happens. Ceres and Vesta are found to be the largest of the asteroids, and their orbits have nearly the same inclination as some of the old planets ; while the orbits of the smaller ones, Juno and Pallas, are inclined to the ecliptic 13° and 34°·5 respectively. Lagrange computed the force of explosion necessary to burst a planet, and convert a portion of it into a systematic wanderer. By the process described in the *Connaissance des Tems* for 1814, he arrived at the conclusion, that were a fragment to be impelled with a velocity equal to 121 times that of a cannon-ball, it would become a *direct* comet, but a *retrograde* one if the velocity were 156 times. With weaker impulse, however, the fragment would describe an ellipse, and thus, it is presumed, the asteroids probably were impelled with only twenty times that velocity. The exact circumstances of these extraordinary bodies are not yet sufficiently determined, and the correction of future observations is urgently necessary ; but the following table, constructed from details in the Nautical Almanac for 1845, exhibits a very close approximation to their principal elements. The planets are arranged in their order of distance from the sun, and in the semi-axes of their orbits ; the semi-axis of the earth's orbit is taken as unity.

Elements.	Vesta.	Juno.	Ceres.	Pallas.
Mean longitude	69 32 15·3	115 43 15·1	327 41 07·8	304 56 26·4
Longitude of perihelion...	251 02 37·4	54 08 33·3	148 14 06·2	121 22 43·5
Long. of ascending node..	103 20 03·4	170 52 28·9	80 48 18·7	172 41 48·1
Inclination to ecliptic ...	7 08 23·2	13 03 05·6	10 37 08·7	34 37 40·2
Angle of excentricity......	5 05 19·9	14 42 23·7	4 32 58·9	13 54 01·2

"Such are the extraordinary conditions of the asteroids, whose intersecting orbits, leading them almost within hail of each other, so to speak, at the rate of more than 40,000 miles an hour, may

eventually lead to mutual disturbances, which the attraction of the larger planets cannot control. Although the strange coincidences attending this group may be *accidental,* in general phrase, yet their phænomena cannot but be considered as evidence tantamount to demonstration, of their having once composed a single planet, and having diverged by the explosive force of a tremendous cataclysm : and in addition to their orbital vagaries, the bodies themselves are not round, as is said to be indicated by the instantaneous diminution of their light on presenting their *angular* faces.''

There is much here to the point, and confirmatory of the theory and facts I am endeavouring to establish, that meteorites belong to the planetary system, and are probably the minute outriders of the group of fragmentary planets called asteroids, or planetoids. It is not improbable that in the course of fifty or one hundred years, supposing due care be used in collecting all information possible regarding the dates and falls of aërolites, and in placing the *data* properly together, that we shall not only readily and certainly determine those epochs, but also the periods in years when the epoch itself again comes round; with such an end in view I have constructed the Table G., but it is not yet sufficiently rich in *data* to admit our arriving at definite results; yet I think there are indications of periods varying from two to five years for some of the months*.

Luminous meteors and falling stars (as well as meteor showers), I would chiefly refer to a class of minute comets, which also no doubt occasionally, like solid meteorolites, enter the earth's atmosphere and are absorbed, but more frequently pass at a moderate distance.

Anything tending to clear up the confusion that exists in the appearances of the meteor class generally, is of interest and value. Some meteoric appearances are doubtless atmospheric and belong to electric or chemical phænomena. It has lately been hinted that others may be mere satellites of our own planet revolving with great velocity and at a very moderate distance. Indeed the phænomena exhibited by these appearances are often so linked together, that one might be ready (too hastily however) to consider them all as one family, the relations of which are not in reality distinguishable from each other.

M. Arago, in his Popular Lectures on Astronomy, appears favourably to view, what is termed the lunar theory, as best adapted to account for the similarity *chemically* existing between meteorites and our own earth.

The Rev. Baden Powell, who has studied this question very

* The planetoids Ceres, Pallas, Vesta and Juno vary from $3\frac{1}{4}$ years to $4\frac{1}{2}$ in their revolution round the sun; Mars takes nearly two years.

attentively, and especially that of luminous meteors, expressed the opinion, at a lecture delivered at the Radcliffe Library, on the 24th June, 1847, that there exists a connexion between aërolites and luminous meteors; and that such small bodies may circulate in the solar system, though probably in small numbers, *unless* truly planetary, or as satellites of some of the larger planets, as of the earth*.

[To be continued.]

XLIV. *On the double Refraction temporarily produced in Isotropic Bodies.* By M. G. WERTHEIM.

[Concluded from p. 261.]

Theory.

LET the quantities H, Lo, La, C, E, *d*, and λ be defined as above. Further, let δ be the linear change which takes place in the direction of the force, the sign of which we will assume to be positive or negative according as the force P produces a lengthening or a shortening; O the velocity of the light in air; O_o and O_e the velocities, ordinary and extraordinary, in the substance, which has become temporarily birefractive.

According to the law of the ratios of linear changes, the three dimensions of the parallelopiped, during the action of the force P, have become

$$H(1+\delta), \quad Lo\left(1-\frac{\delta}{3}\right), \quad La\left(1-\frac{\delta}{3}\right).$$

The two rays have to pass over the length $Lo\left(1-\frac{\delta}{3}\right)$, consequently their difference of path *d*, after issuing from the parallelopiped, is proportional to $Lo\left(1-\frac{\delta}{3}\right)\left(\frac{O}{O_o}-\frac{O}{O_e}\right)$ and to the linear change δ; we have thus

$$d = Lo\delta\left(1-\frac{\delta}{3}\right)\left(\frac{O}{O_o}-\frac{O}{O_e}\right).$$

The value of δ can always be determined by means of the coefficient of mechanical elasticity; for pretty considerable charges

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* It has been shown by Erman and Pierce (See American Philosophical Transactions for 1841), that the influence of the earth's attraction on meteoric bodies, approaching near that planet, with planetary velocity, is *not* considerable; at least not equal to any errors of observation in a calculation of their orbits. It has also been proved that the maximum velocity of a meteoric body, revolving, as a *satellite of the earth*, cannot exceed 5½ miles in a second, whereas the average velocity of these bodies is about fifteen miles per second.

we have

$$\delta = \frac{P}{E \cdot Lo \cdot La};$$

for feeble charges, we can construct upon the curve the ratio of the veritable ordinate to that which would be furnished by the coefficient of elasticity which corresponds to the dotted line.

But neglecting these small differences, and remarking that for all the substances which we have examined, δ may always be neglected in comparison with unity, we obtain, by substituting in the preceding equation the value of δ, the equation

$$\pm d = \frac{P}{E \cdot La}\left(\frac{O}{O_o} - \frac{O}{O_e}\right), \quad \cdots \quad (1)$$

which contains our propositions 1, 2, 3 and 5.

$\dfrac{O}{O_o}$ and $\dfrac{O}{O_e}$ are the two indices of refraction I_o and I_e; and for $d = \pm 1$ and $La = 1$, we have $P = C$, and consequently

$$I_e = I_o \mp \frac{E}{C}, \quad \cdots \quad \cdots \quad (2)$$

where it is necessary to take the sign $-$ for traction, and the sign $+$ for pressure. It now remains to find the values of I_o and E.

M. Dutirou has determined the indices of refraction of the glasses derived from the same manufactories as those which I have employed; I might therefore have made use of his results. But notwithstanding the identity of origin, the densities of my glasses differ notably from those found by M. Dutirou. These differences indicate corresponding differences in the composition, and I have thought it necessary, wherever such differences appeared, to ascertain by direct experiment the refractive index of the substance. For this purpose a prism was cut from the mass from which the corresponding parallelopiped was taken, and by means of the goniometer of M. Babinet, the index for the mean yellow ray was determined; this suffices in the case before us, the difference $I_e - I_o$, which it is our object to find, being independent of the length of the undulation. Fluor-spar and rock-salt are bodies of such constant properties, that I have been able with safety to make use of the indices given by Sir David Brewster; but I have deemed it necessary to determine the index of inactive alum, on account of the peculiar properties which it exhibits.

The coefficient of mechanical elasticity has been found for each substance by means of the transversal vibrations of a thin plate, sufficiently long, which we were careful to take from the

side of each of the parallelopipeds. Put in vibration by means of a bow, the two ends being free, each plate gave its fundamental tone and some of its harmonics. Experiments have been recently published which tend to cast some doubt on the correctness of this method, and on the certitude of the results derived from it. But by means of experiments on strips of copper of various lengths and thicknesses, which I have caused to vibrate transversely, after having previously determined the elasticity by means of direct elongation, I have assured myself anew of its accuracy; only it is necessary to take care not to fasten one of the ends of a short plate when transversal vibrations are required : the fastening of the end causes, as is known, errors so considerable as to alter completely the results of the experiments.

The values of E which have been obtained by this method, have been afterwards divided by the coefficient of correction 1·05*, in order to find those which would have been found by the way of elongation.

I have caused a plate to be cut for each of the parallelopipeds; I have hence three plates of rock-salt from different sources, several of fluor-spar, &c. For plates of the same kind, the co-efficients of elasticity differ so little among themselves, that I have only found it necessary to inscribe the means in the following table, reserving to myself to publish the numbers themselves in a special memoir on the elasticity of crystals.

Substances.	Density.	Coefficient of elasticity.		$\frac{E}{C}$	Index of ordinary refraction.		Index of extraordinary refraction.	
		Mechanical. E.	Optical. C.		l_0.	Authors.	Double refraction.	
							Negative.	Positive.
Crown of MM. Maës and Clémandot...	2·657	5888	26978	0·2182	1·532	W.	1·750	1·314
Crown of M. Feil......	2·629	6397	29923	0·2138	1·541	Dutirou.	1·755	1·327
Flint Guinand	3·589	4976	25917	0·1920	1·617	W.	1·809	1·425
Plate glass	2·457	6180	32396	0·1908	1·543‡	Wollaston.	1·734	1·352
Ordinary crown	2·447	6220	34714	0·1792	1·517	Dutirou.	1·696	1·338
Flint of MM. Maës and Clémandot...	3·538	5323	32593	0·1633	1·614	W.	1·777	1·451
Fluor-spar	3·183	8647	55605	0·1555	1·436	Brewster.	1·591	1·281
Rock-salt	2·136	3876	38483	0·1007	1·557	Brewster.	1·658	1·456
Borosilicate of lead...	4·050	5208	52031	0·1001	1·676	W.	1·776	1·576
Heavy flint of M. Feil	4·056	5017	50932	0·0985	1·624	Dutirou.	1·722	1·526
Faraday's flint	4·358	5017†	57332	0·0875	1·681	W.	1·768	1·594
Inactive alum	1·632	975	15197	0 0641	1·455	W.	1·519	1·391

* *Ann. de Chim. et de Phys.* 3 sér. vol. xxxi. p. 39.

† The density 2·329 of the plate glass employed by M. Dutirou, differs too much from that of my glass to permit of my assuming the index found by this physicist.

‡ Having been able to procure only a single cube of the glass, I have

The quantity $I_e - I_o = \mp \dfrac{E}{C}$ is the true measure of the double refraction; for if we take as such the quantity $I_e{}^2 - I_o{}^2$ or $\dfrac{I_o{}^2}{I_e{}^2}$, we should obtain for the same substance a different birefractive power according as it happened to be temporarily positive or negative, which is inadmissible.

It might be hoped that these researches would conduct to some simple ratio between the birefractive power which may be called *specific,* and the other properties of the body. My first experiments, which were made with some specimens of glass only, had furnished a sensibly constant value for the birefractive power; and if this constancy had been general, important consequences relative to the distribution of force in bodies naturally birefractive would have flowed from it. But since these first experiments, I have observed* that heavy flint has an extremely high coefficient of optical elasticity; the ratio of the two coefficients could not therefore be the same as for the other glasses, unless the flint possessed a very high coefficient of mechanical elasticity also; this is hardly probable, when we consider the great quantity of lead which enters into its composition. Indeed, experiment has demonstrated the contrary, and the results which I have since obtained on the double refraction produced in isotropic crystalline bodies are in direct contradiction with any theory of this kind.

The birefractive power does not stand in any simple ratio to the density; neither is it a function of the velocity O_o of light in isotropic bodies, as might be inferred from the results which M. Broch has obtained by calculation†.

We are thus obliged to assume that the birefractive power, like the refractive, is inherent in each substance, or what is the same thing, that it depends upon a relation still unknown between the mechanical and optical elasticities of the body.

Further, to be sure that there is no parallel between the two kinds of double refraction,—the natural and the artificial, it is sufficient to consider the magnitude of the forces which it would be necessary to apply to an isotropic body to cause it to produce, with equal thicknesses, a difference of path equal to that which results from the passage across a plate parallel to the axis of certain birefracting crystals. Let us compare, for example, Iceland spar with ordinary crown glass; the difference of the two

been obliged to suppose its coefficient of elasticity equal to that of the heavy flint preceding; but its great density renders it probable that the coefficient is really a little more elevated.

* *Comptes Rendus,* vol. xxxii. p. 291.
† Dove, *Répertoire de Physique,* vol. vii. p. 58.

indices of refraction is sensibly the same for the two substances. For the plate of spar we have

$$d = L_o(I_e - I_o) ;$$

substituting this value in the equation (1), we find

$$P = E . Lo . La ;$$

and for the unit of surface,

$$P = E.$$

It would therefore be necessary to apply to the plate of crown a pressure of 6220 kilogrammes to each square millimetre of the transverse section perpendicular to the thickness; that is to say, a pressure more than a thousand times greater than that which would crush the glass to pieces.

It now remains to compare the value of the double refraction which we have just found for plate glass, with that which results from the investigations of M. Neumann. After having shown that the temporary optic axes are represented by the following expressions,—

$$A = O' + p\alpha + p\beta + q\gamma ; \quad B = O' + p\alpha + q\beta + p\gamma ;$$
$$C = O' + q\alpha + p\beta + p\gamma,$$

in which the quantity O' differs infinitely little from the velocity O_o of light in isotropic bodies, and where α, β, γ represent the proportional changes of length in the directions of the three mechanical axes; M. Neumann seeks to determine the values of p and q.

To find two equations between p and q, he employs two different processes, but both of them based on the use of formulæ generally admitted for the flexure of prismatic bodies.

In the first process, we determine in the medial plane the flexure assumed by the band of curved glass, and the distances of the neutral axis from two points which possess the same tint, corresponding to a certain thickness of air as determined by the table of Newton; of these two points, one is above the neutral axis in the compressed portion of the band, and the other at an equal distance below the axis in the dilated portion.

Assuming that a very small parallelopiped, placed at one of these points, will suffer the same linear changes as if it had been compressed or dilated by a force equal to that which results from the flexure, we can find, by known formulæ, the relation between the double refraction and the mechanical linear change; we have hence the value of $p - q$.

The second process is based on the method of displacement of fringes, as in the experiment of M. Arago. Of two rays which interfere, one has passed through the dilated, and the other

through the compressed portion of the curved glass; the two systems of fringes, extraordinary and ordinary, which are observed with a birefracting prism, are displaced with reference to one another, and move as a vernier on its scale. The measure of the displacement gives a second equation between p and q.

But in order to deduce numerical results from these experiments, it is necessary to assume a determinate ratio between the changes of length which occur in the directions of the three mechanical axes. In theory, this ratio, or what is the same thing, the law of change of volume, may rest indeterminate, as asserted by MM. Lamé and Maxwell*; but not so when it is the question of an experiment, the results of which we would know and apply.

M. Neumann has found himself compelled to adopt the law of Poisson, the accuracy of which had not indeed been contested when his memoir was published; consequently we have

$$\gamma = \pm \delta; \quad \alpha = \beta = \mp \frac{\delta}{4},$$

and we find

$$p - q = 0.054; \quad p = -0.085; \quad q = -0.139.$$

Introducing the new law into the formulæ of M. Neumann, we see that all the numerical coefficients are simplified, and we obtain at the end of the calculation the following values :—

$$\gamma = \pm \delta; \quad \alpha = \beta = \mp \frac{\delta}{3}; \quad p - q = 0.0505;$$

$$p = -0.3006; \quad q = -0.3511,$$

which differ notably from the old ones. If, as in the present memoir, we confine ourselves to the determination of the quan-

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* I profit by this occasion to protest against the inexact manner in which Mr. Maxwell has reported my experiments (Trans. of the Roy. Soc. of Edinburgh, vol. xx. part 1, page 87). "M. Wertheim," he says, "has given the results of some experiments on caoutchouc, by means of which he finds $K = k$ or $\mu = \frac{4}{3}m$, and he concludes that for all substances we have $K = k$."

Here Mr. Maxwell speaks only of experiments which I expressly stated to be preliminary ones, and he passes in silence others much more exact and more varied, which I have made by means of hollow cylinders of different substances.

To demonstrate afterwards that the law of the change of volume may vary with the nature of the body, Mr. Maxwell cites cork, which has a cubical elasticity smaller, and a linear elasticity greater, than the corresponding elasticities of a jelly. It will certainly not be admitted without difficulty that these two substances are types of homogeneous bodies; but even if they were, Mr. Maxwell has cited no experiment in support of his opinion.

tity $\frac{E}{C}$,—which corresponds, in the formulæ of M. Neumann, to $\frac{2\delta}{\gamma}$,—we find it equal to 0·157 by the old formulæ, and equal to 0·168 by the new ones; this latter differs much less from the value 0·191 which we have obtained by direct experiment. But the difference becomes still less when we take into account that M. Neumann has applied his calculation to the means of measurements taken on two bands which were not of the same description of glass: further, in their composition the glasses of Germany employed by M. Neumann differ sufficiently from those of France to prevent the values of E and of C from remaining the same. A band of Bohemian glass has given me, by means of transversal vibrations, a coefficient of mechanical elasticity of 6594, which is notably superior to that of our glasses.

In general, the theory of flexure itself, and the determination of the changes of volume which occur in all the parts of the body submitted to flexure, seem to me to be based on too many hypotheses with reference to the position of the neutral axis, to serve for the calculation of experiments so delicate as the present; by following an inverse method, it is my intention to make use of the results which, by purely direct means, I have obtained in this memoir, on the examination and elucidation of the theory of flexure.

Simultaneous effects of a mechanical force and of Magnetism.

The proximity of a powerful magnet produces in certain isotropic bodies a rotation of the plane of polarization. It was interesting to see how this novel action of magnetism, which has been discovered by Mr. Faraday, would be modified when the same body had ceased to be isotropic, and had acquired optic and mechanic axes of different magnitudes. MM. Bertin and Matteucci have made already some researches in this direction.

It has been seen how the apparatus is disposed for these experiments, which have been carried out principally on substances endowed with the rotatory magnetic power, such as the flints. The result has been the same for all substances; the rotation disappears according as the axes become unequal; a relatively feeble pressure or traction is sufficient to render the phænomenon less defined, and afterwards it becomes more weakened as we augment the charge; but the moment when it disappears cannot be exactly determined, because the operations lose much of their precision when, in consequence of the newly-produced double refraction, the two images become illuminated. By making use of homogeneous rays, we may in all cases convince ourselves that the rotation has completely disappeared when the difference

of path has amounted to $\frac{\lambda}{2}$, for then the ordinary image is completely obscure; and if the least trace of rotation existed, it would manifest itself immediately in this image. The insensibility remains the same, whether we establish the current in one or the other direction, or interrupt it, or even when we reverse the poles of the magnet.

It is to be observed, that the glass endowed with the most energetic rotatory power is at the same time that in which the birefracting power is most feeble; the same observation applies, among isotropic bodies, to alum and rock-salt, and a similar analogy appears to exist in bodies naturally doubly refractive: the rotation is zero in Iceland spar, the birefractive power of which is 0·175, while it is very sensible in quartz, which possesses the very feeble refractive power of 0·009. This is a point which it will be necessary to take into account in subsequent researches; for the present it is enough that we have demonstrated that this apparently direct action of the magnetism upon the æther depends essentially on the constitution of the latter, and that it can be annulled by forces purely mechanical.

Applications of the temporary double refraction.

The formula (1) contains all the quantities which exert an influence on the phænomenon of double refraction: thus far we have made use of it to determine the value of the indices of extraordinary refraction; but when those indices are once known, the same formula can serve for the determination of any one of the quantities contained in it.

A. *Determination of the force* P ;—*Chromatic Dynamometer.*

The dynamometers designed to measure the effects of traction, have, during the last few years, been carried to a high degree of perfection by the labours of MM. Poncelet and Morin; but this is not the case with regard to the measurements of pressures exerted between two solid bodies; no exact instrument has as yet been constructed for the purpose; we know nothing of the useful effect of the commonest machines, such as presses, vices, systems of levers, &c.

The chromatic dynamometer (Plate II. fig. 3) seems to me to supply this want; after what has been said, it will be easy to understand its construction and application. The essential part is a plate of glass, perfectly transparent in the direction of its length, of such dimensions that it can support very considerable pressures, and blackened all round with the exception of the two faces through which the operator is to look. This plate of

glass *a*, furnished on its two parallel surfaces with parallel cards of vulcanized caoutchouc, is placed between two surfaces of cast iron well planed and sufficiently thick; the inferior of these plates *b* carries two tubes of brass, the interior surfaces of which are blackened. The object-tube contains a Nichol *d* at the extremity nearest to the observer, and carries at the other end a plate of white porcelain *e*, which is moveable in two perpendicular directions, and which, consequently, can always be placed in a position to be well illuminated. This arrangement has the advantage of permitting those rays only to arrive at the Nichol which are sensibly parallel to the axis, and to cut away the internal reflexions, which are always prejudicial; the other tube *f* carries a birefracting prism *g*. These two tubes are mounted stiffly in the grooves *h*, adapted to the inferior plate, so that by employing plates of glass of different thicknesses, we can always place their axes in the prolongation of each other, and at half the height of the glass. The upper plate of metal *i* is altogether free; it is placed upon the last card, and serves simply to transmit to the glass the pressure it receives, without being able by any friction whatever to cause a loss of force. It will be further understood, that the exterior diameter of the tubes ought always to be smaller than the sum of the thicknesses of the horizontal plates.

The principal sections of the Nichol and the birefracting prism are parallel to each other, and form an angle of 45 degrees with the vertical.

We have now only to place this small apparatus between the two surfaces on which the pressure is to be exerted. Whatever be the nature of the force applied, whatever be the losses which it may have sustained before producing the useful effect, the measure of this effect will be always given by the simple inspection of the colours which exhibit themselves in the two images, that is to say, by the value of *d*. We have, in fact, the equation

$$P = \frac{d \cdot E \cdot La}{I_e - I_o}.$$

The observer can free himself from all calculation: for this purpose I have caused to be painted the forty-five colours contained in our table, on a black ground, so that they may be seen in the instrument; by the side of each couple are inscribed the corresponding values of P for two plates of glass, one of which is destined to measure feeble pressures, while the other can support more considerable ones. The experiments present but one difficulty, which is to be avoided by preventing the transverse flexure of the glass; for the latter would cause the coloured bands of Fresnel to exhibit themselves, and then all exact determinations would become impossible. This cause of disturbance

presents itself infallibly whenever we make use of thin plates ; it is impossible to render the surfaces between which the pressure is exerted so completely plane and parallel that they shall rigorously produce nothing more than an effect of compression. It is therefore necessary that the glass itself offer sufficient resistance to flexure to cause its effect to become insensible, and this is attained by giving the plate a thickness (height) of from 2 to 3 centimetres. It is hardly possible to obtain by *refoulement* so thick a glass ; on the other hand, the cast glasses always present a great number of striæ in a direction perpendicular to their pressure, which is the precise direction in which they would be traversed by the light. I have had recourse to the juxtaposition of several layers of plate glass placed one upon the other ; these layers, after having been united by Canada balsam, have been cut in such a manner as to form only a single thick plate, perfectly transparent, and which fulfills all the required conditions. It is easy to ascertain the dimensions which it is necessary to give to such a plate, to render it fit to measure a force of any given magnitude. Plate glass supports a pressure of 3 kilogrammes per square millimetre ; a square plate of 20 centimetres the side, could therefore, without inconvenience, be submitted to a pressure of 120,000 kilogrammes. But long before it attains this limit the two images would become completely white, in consequence of the great difference of path between the two rays.

In fact, according to our table, the greatest difference of path which could be made use of is 0·002 of a millimetre ; the charge corresponding is given by the formula

$$P = \frac{0 \cdot 002 \times 6180 \times 200}{0 \cdot 191} = 12{,}942 \text{ kilogrammes,}$$

or in round numbers, equal to 13,000 kilogrammes, a limit which could not be exceeded without employing a larger plate.

But without having recourse to a plate of larger dimensions, which would at the same time diminish the sensibility of the instrument, we can augment its power by a very simple artifice. Suppose that we have arrived at a pressure of 13,000 kilogrammes; the extraordinary ray would be in advance of its ordinary a distance of 0·002. Let us now place in the line of the rays between the two prisms (at *l*, fig. 3), a plate of quartz cut parallel to the axis, and of such a thickness that of itself it could produce a difference of path of 7λ, or of 0·004. The quartz is a positive crystal ; and hence by adding the plate to the compressed glass, we change the sign of the difference of path *d* ;—from −0·002 it becomes +0·002. The colours of the two images have therefore suffered no change ; but by now augmenting the pressure, they will gra-

dually ascend through the whole series of tints, and with a press-ure of 26,000 we shall come to zero. Consequently, and with-out adding anything to the optical apparatus, we can proceed anew to a negative double refraction of $\frac{7\lambda}{2}$, which would be ob-tained with a total pressure of 39,000 kilogrammes, and so on.

This apparatus appears to me capable of being employed in a great number of scientific experiments, and it would enable engineers to graduate in a rigorous manner manometers, safety-valves, and other instruments of measurement which are ha-bitually applied.

I will confine myself to citing a few examples.

With two small screws turned by the fingers merely, we can produce a pressure of 220 kilogrammes.

To copy a letter with an ordinary copying-press, a pressure of 800 to 900 kilogrammes is usually exerted.

A large press, in the workshop of M. Breguet, has given the results contained in the following table :—

P'.	P.	Ratio of P to P'.	P'.	P.	Ratio of P to P'.
k			k		
0·0	66·5	P = 0·0 +66·5	14	699·6	P = 45·2 × P′+66·5
2·0	159·6	P = 46·6 × P′+66·5	15	824·6	50·5 ...
3·0	207·5	47·0 ...	17	941·6	51·5 ...
5·0	319·2	50·5 ...	20	1057·4	49·5 ...
7·5	425·0	47·8 ...	25	1353·9	51·5 ...
10·0	558·6	49·2 ...	28	1468·3	52·2 ...
12·0	622·4	46·3 ...			
Mean............ P = 48·98 × P′+66·5					

We have denoted by P′ the force which is applied to one arm of the lever, and by P the effect produced; the constant quan-tity 66·5, which is independent of the force P′, represents the *inertia of the instrument* and the friction in the screw which corresponds to this inertia : if, after having applied any force whatever P′, we remove the force, the pressure will not entirely disappear ; a coloration still exists corresponding to a pressure of 66·5 kilogrammes, which evidently arises from the inertia of the apparatus. We see, then, that this force, which is generally neglected, may in certain cases become very considerable.

Through the kindness of M. Morin I have been also able to make a series of experiments with the great hydraulic press with four cylinders, which is placed in the Conservatoire des Arts et Métiers ; it is furnished with a manometer which communicates directly with the water of the press ; so that the theoretic and effective pressures may be compared with each other. The

sum of the surfaces of the four pistons is 181·456 square centimetres.

	Pressures.	
By the chromatic dynamometer in kilogrammes.	By the manometer.	
	In atmospheres.	In kilogrammes.
100	0·5	91
175	1·0	181
283	1·6	290
383	2·1	381
514	3·0	544
555	3·5	634
586	4·0	726
938	6·0	1088
1285	8·0	1451
1523	9·0	1633
1666	10·0	1814
2464	15·0	2721
3091	20·0	3629
3338	24·0	4355
3427	28·0	5080
4099	31·0	5625
4480	35·0	6351
4950	40·0	7258
5891	47·0	8528

The first numbers were obtained with the piece of flint No. 13, and it was not until 1000 kilogrammes had been reached that we made use of the larger piece of plate glass. The only difficulty consists in always having two plates, one fixed and the other moveable, well cut and parallel so as to exhibit no sensible flexure. The results have then all the exactitude that can be desired, and they show how great are the errors which we commit when we imagine that we can neglect all the frictions in our calculations of the useful effect of the hydraulic press.

B. *Determination of the coefficient of elasticity.*

We have

$$E = \frac{P\,(I_e - I_o)}{d\,.\,La}.$$

This determination would not offer any difficulty if the index of extraordinary refraction could be found directly, or if it were a known function of I_o, the ordinary index; but, in the actual state of matters, this formula can only serve for approximate estimates in cases where all other means fail, as, for example, in determining the elasticity of precious stones. I have attempted to apply it to the diamond, which presents a double interest, both as a simple and dimorphous body, and as the hardest of all known

substances. I have subjected to compression a parallelopiped, very little different from a cube, cut from a perfectly pure diamond, and of which two pairs of faces were exactly parallel to each other; the three dimensions of this parallelopiped are 2·847, 2·874, and 2·760 millimetres*. After having eliminated, as much as possible, the effect of the very energetic laminar polarization in this diamond, we find as means of six experiments which were made by taking successively each of the dimensions for height, for width and for length, the following numbers:—

$$\frac{P_\lambda}{La} = 11\cdot20 \text{ kilogs.}; \quad \frac{P_{2\lambda}}{La} = 11\cdot09 \text{ kilogs.}; \quad \frac{P_3}{La} = 12\cdot50 \text{ kilogs.};$$

and finally, the coefficient of optic elasticity $C = 43985$; we have besides $I_o = 2\cdot470$ (Brewster), but the value of I_e is unknown. Let it be remarked, however, that for all the bodies which we have examined, the value of the ratio $\frac{I_e - I_o}{I_o}$ varies between 0·04 and 0·14; hence, assuming for the diamond a value equal to that of fluor-spar, or in round numbers, equal to 0·1, we shall have $I_e - I_o = 0\cdot247$ and $E = 10865$. The coefficient of elasticity of diamond would thus be nearly equal to that of copper, and not at all in proportion to its great hardness.

C. *Determination of the length of an undulation of a given light.*

We have already remarked that these experiments furnish us with a ready and precise means of recognizing whether the light in question is homogeneous, and of determining the heterogeneous rays which are mixed with the principal colour. We have also shown how the length of an undulation of any ray whatever may be found, when once we have determined for a homogeneous ray of known length, the charge which, applied to any isotropic substance, produces an extinction of any order whatever of one or the other image.

Conclusions.

1. The double refraction produced artificially, whether by traction or compression, is, for the same substance, proportional to the linear changes which this force produces in the directions of the principal axes, and consequently proportional also to the change of volume of the body.

2. The temporary lengthenings and shortenings produced by a given weight, according as it acts by traction or by pressure,

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* M. Halphen has had the extreme kindness to have this diamond cut and polished in his manufactory; everybody will comprehend the difficulties which he had to surmount to obtain the parallelism of the faces.

are not rigorously equal to each other, nor exactly proportional to the weights, as long as the latter are small; but these differences disappear as the charges become more considerable, and long before any sensible permanent changes have been produced.

If we take the weights as abscissæ, and the lengthenings or shortenings corresponding as ordinates, the first below, and the second above, two similar, if not equal curves, will be obtained, the first of which is convex, and the second concave towards the axis of the abscissæ; the curves straighten insensibly, and, for linear changes hardly measurable by ordinary means, become confounded with a straight line, which proves the proportionality between the charges and their temporary effects. These facts are confirmed by direct experiments made by different observers, experiments the results of which were however too uncertain to be regarded as a demonstration of the law. This confirmation is exhibited in a notable degree by the experiments of Mr. Hodgkinson, when they are calculated with reference to the temporary effects alone, and when we take into account the sources of error which influence all direct experiments by means of compression.

3. The optic axes coincide with the mechanical ones for all really isotropic bodies, whether these bodies have been endowed with a negative double refraction by means of pressure, or with a positive double refraction by means of traction. The double refraction, or the difference of path between the two rays, ordinary and extraordinary, may be determined very exactly by means of the complementary colours assumed by the two images of a white beam, when the principal section of the polarizing Nichol and the analysing birefracting prism form an angle of 45 degrees with the direction of the force applied to the body placed between these two prisms. In both cases the colours ascend with the charges, following exactly the series of the coloured rings of Newton; but, for actual measurements, we can hardly make use of more than the colours of the first seven half rings: the colours of the transmitted rings are those of the ordinary image, while the tints of the reflected rings correspond to the extraordinary image.

4. Neglecting the small differences which we have just referred to, the temporary double refraction is independent of the height and length of the piece, proportional to the weight applied and to the birefractive power of the substance, and reciprocally proportional to its width and to its coefficient of mechanical elasticity.

5. The birefracting power of an isotropic substance, which has become temporarily birefractive, can only be expressed by the difference between its ordinary index and its extraordinary one;

this difference only changes the sign, according as we apply pressure or traction; this could not take place if the birefracting power were expressed as any function of the two indices other than the difference of the first powers.

6. The dispersion of the double refraction is insensible for the substances which we have submitted to experiment.

7. The glasses which have been subjected to the process of *refoulement* have ceased to be optically homogeneous bodies; and this alteration, entirely distinct from that which we call the *temper of the glass*, does not always disappear by annealing.

8. The birefracting power is not the same for different isotropic substances; no relation can be established between this power and the index of ordinary refraction, or even the density.

9. By analogy with the coefficient of ordinary or mechanical elasticity E, we apply the name of coefficient of optic elasticity to the ratio of the charge applied to the unit of surface and the double refraction which it produces; we have then the simple equation

$$I_o - I_e = \frac{E}{C},$$

which serves to determine the birefractive power,

$$p = \pm (I_o - I_e).$$

10. The value of the birefracting power of any substance being once known, the phænomena of double refraction may be made use of to determine any one of the quantities which enter into the equation

$$\pm P(I_o - I_e) = d \cdot E \cdot La.$$

11. The most important of these applications consists in determining the force P, whatever be its magnitude or mode of action. The chromatic dynamometer gives immediately, and without the employment of any coefficient of correction, the effective pressure of a vice, press, hydraulic press, lever, &c.; it can be made use of to ascertain in all machines the ratio between the useful and the theoretic effect, to graduate in an exact manner ordinary manometers, and even to measure *vis viva*.

12. The same formula would serve to determine the coefficient of mechanical elasticity, if we possessed the means of finding directly the index of extraordinary refraction I_e; but, in the mean time, it has enabled me to establish the optical coefficient of the diamond, and to fix certain limits between which its mechanical coefficient is comprised.

13. The difference of path d being independent of the wavelength λ, if the ratio $\dfrac{d}{\lambda}$ remains the same for different values of

λ, P must vary as λ; this furnishes a ready means of determining the lengths of the undulations, and of deciding whether a given light is homogeneous, or what are the different rays which compose it.

14. The phænomena of magnetic rotation disappear in substances according as they cease to be mechanically homogeneous and optically isotropic ; it is to be remarked, that among naturally birefractive bodies, or bodies rendered so by the application of a mechanical force, those which possess the most energetic powers of rotation are at the same time endowed with the feeblest powers of double refraction.

XLV. *On the Expansion of certain Substances by Cold. By* W. J. Macquorn Rankine, *Civil Engineer, F.R.SS. Lond. and Edinb. &c.**

DURING the discussion which followed the reading of the Rev. Prof. Powell's Report on Radiant Heat, at the late meeting of the British Association, it appears, from the report in the *Athenæum*, that a question was proposed by Dr. Stevelly as to the means of reconciling the dynamical theory of heat with the fact of the expansion of water, antimony, cast iron, and bismuth, by cold, at and near their freezing-points; and that some observations were made in reply by Professor Powell and Prof. William Thomson.

The question is one of much importance, and calls for the attention of every one who has been instrumental in maintaining the dynamical theory of heat. Having been prevented by illness from attending the meeting of the Association, I beg leave to offer the following remarks for publication in the Philosophical Magazine.

The theory of thermo-dynamics, strictly speaking, is a system of propositions, all of which are deducible from the following two laws :—

I. Heat is convertible with mechanical power.

II. The mechanical equivalent of the quantity of heat which disappears or appears during any indefinitely small change (dV) of the volume of a given mass of a given substance, at a constant absolute temperature (τ), is the product of the following three quantities : the variation of volume, the absolute temperature, and the rate of variation of the expansive pressure (P) with temperature at constant volume; that is to say,

$$\tau \frac{d\mathrm{P}}{d\tau} d\mathrm{V}.$$

* Communicated by the Author.

When this product is positive, it represents heat which disappears; when negative, heat which appears.

Neither of those laws indicates any particular relation between the pressure, volume, and temperature of a given mass of a given substance as more probable than any other. The nature of such relations must be determined for each substance by experiment, before the two general laws above stated can be applied to it.

Hence it appears that it is not the province of the theory of thermo-dynamics to explain the fact of the expansion of certain substances with cold, but simply to deduce the consequences of that fact, so far as they relate to the quantities of heat which are made to appear and disappear by effecting given changes in the volume of those substances*.

It is true that if we frame a *hypothesis* as to the molecular structure of matter (such as the hypothesis of molecular vortices), so as to deduce the two laws of thermo-dynamics from those of ordinary mechanics, that hypothesis must lead to many consequences besides those two laws; and it is necessary that those consequences should not be inconsistent with any of the phæ-nomena of the relations between the temperature, volume, and elasticity of bodies. No such inconsistency has hitherto been proved; but even were such inconsistencies to be proved, no objection to a molecular hypothesis can affect the certainty of the two laws of thermo-dynamics, which, though at first anticipated as the results of a hypothesis, have now been independently established by experiment.

With respect to the molecular mechanism that may be supposed to give rise to the expansion of certain liquids in cooling, near their freezing-points, it appears to me that, so far as our present knowledge enables us to judge, the most probable view is one concurred in by Professor Thomson, viz. that in a liquid near its freezing-point, there is an incipient tendency of the particles to assume a definite arrangement with respect to certain fixed axes; and that in substances which expand in freezing, that arrangement is such as to make the particles occupy more space than they do in the perfectly liquid state.

Glasgow, October 10, 1854:

* One of those consequences was anticipated by Mr. James Thomson, and proved experimentally by Professor William Thomson, at a time when the theory of thermo-dynamics was in a very imperfect state, viz. that the freezing-point of water is lowered by pressure.

XLVI. *On the May Hill Sandstone, and the Palæozoic System of England. By the* Rev. Prof. ADAM SEDGWICK, *F.R.S., F.G.S.*

[Concluded from p. 317.]

Tabular View and Nomenclature of the British Palæozoic System, &c.

AS a sequel to the previous details, I subjoin the following remarks on the classification and nomenclature of the British palæozoic rocks. The triple division of our whole series of fossil-bearing rocks into *primary, secondary,* and *tertiary,* may still be conveniently retained; and, when these three divisions are described with reference to their organic types, they have been respectively defined by the terms *palæozoic, mesozoic,* and *cænozoic.* To each of the three great divisions I would, on palæontological grounds, give the name of *System.* Thus, in the following corrected tabular view, all the palæozoic rocks, from the oldest Cambrian to the Permian inclusive, are described as forming parts of one system—the Palæozoic. This extended application of the word *system* is an innovation on a nomenclature now in common use; but it was suggested by myself fourteen years since, for the express purpose of avoiding what I thought an unphilosophical use of the word *system,* as applied to the subdivisions of the palæozoic rocks—such as Carboniferous, Devonian, Silurian, &c.; and since then I have repeatedly used the word *system* in this extended sense, and recommended its adoption*. Strictly speaking, all organic types, of whatever date, belong to one great *Systema Naturæ.* Had this not been true, palæontology could never have risen into a science. Still there is such a great organic interval between the *flora* and *fauna* of the primary and secondary groups, that we may conveniently, and without any risk of error, describe each of them as belonging to a separate organic system. A single glance convinces us that the *fauna* of the secondary period greatly differs from that of the primary or palæozoic. But the case is widely different in comparing the organic types of the subdivisions of the palæozoic rocks. Both physically and palæontologically, we find a great difficulty in drawing fixed lines between these subdivisions; nor at this moment are geologists in all cases agreed as to the true places where such lines should be drawn.

Not only have all the palæozoic rocks a peculiar organic type, but there are several species which run through nearly all the subordinate groups, and seem, therefore, to unite them into

* For example, Proceedings of the Geological Society, vol. iv. June, 1843, p. 221.

one palæozoic system.   For example, *Favosites alveolaris,* and
F. Gothlandica, Stenopora fibrosa, Spirigerina reticularis, and
Lepagonia depressa are all found in the Bala group; yet three
of them rise into the Devonian series, and the remaining two
rise into the Carboniferous.   And this list of common species
might, I doubt not, be considerably increased.   There is, then,
no incongruity, and certainly no risk of error, in applying the
term *system* to all the palæozoic rocks; while there is some in-
congruity, and (as we know from experience) a great risk of
error, in applying the definite term *system* to great groups of
rocks, such as the Carboniferous, Devonian, and Silurian.   The
use of the word *series,* in describing the subdivisions of the
palæozoic system, is by no means an innovation; but is, on the
contrary, a return to the language formerly in very common use
among the members of the Geological Society; as any one may
see on turning over the pages of their early volumes.

Among the most exact continental writers, the word *ter-
rain* is used for collective, and sometimes imperfectly-defined,
groups of rocks; while the word *système* is applied to the sub-
ordinate and well-defined groups, whether natural or artificial.
This sense of the word *system* is consistent and philosophical,
and can seldom lead to mistakes of interpretation.   But in Eng-
land the word *system* has frequently been used in a loose and
ill-defined sense; sometimes being applied to certain well-defined
groups, on evidence partly physical and partly palæontological;
sometimes to utterly undefined groups, on evidence purely palæ-
ontological.   And hence has been introduced an incongruous
nomenclature, which is sometimes geographical, sometimes palæ-
ontological, and sometimes neither geographical nor palæonto-
logical.   By adopting a definite nomenclature before we are
well acquainted with the physical and palæontological phænomena
on which it must ultimately rest, we inevitably retard the pro-
gress of geology, and damage its scientific language.   For no
one who has prematurely adopted a definite nomenclature is
ever very willing to change it; and it is notorious, that men are
far more ready to distort new facts so as to fit them to an
accepted nomenclature, than to modify and improve their
nomenclature so that it may meet the demands of advancing
knowledge.

The rocks of England are now so well known, that a pretty
good classification of them (from the oldest Cambrian to the
newest Tertiary) might be grounded on physical evidence only.
In like manner (so soon as the long succession of physical
groups is known) a good classification of the whole English
series might be made on purely palæontological evidence.   The
one would represent a vast succession of physical conditions, the

other a similar succession of organic types, and each might have its appropriate nomenclature. Nature has been so true to her workmanship, that the two would, with a very few exceptions, run harmoniously together. But in the actual state of our information, we have done well constantly to use both kinds of evidence; and on both together has our best, and our only consistent and secure, nomenclature been founded. If it be true that experienced geologists have sometimes, from the neglect of palæontology, blundered in the arrangement of physical groups, it is equally true, that very expert palæontologists, from some mistake of arrangement among the physical groups, have sometimes blundered in their palæontology. The double kind of evidence above described, has seldom been lost sight of with impunity.

We have, within a few years, had an excellent illustration of the combined value of physical and fossil evidence, in the definition of our oldest tertiary groups, and in the determination of their nomenclature by Mr. Prestwich; and principles precisely similar have led to a good geographical nomenclature of the great subdivisions of our whole palæozoic system. So far as our palæozoic nomenclature is good, and fitted to endure, it has been grounded both on physical and palæontological evidence. So far as it is inadequate or erroneous, it has either been derived from sections which were misinterpreted, or from a positive desertion of those principles of combined evidence on which all the good and enduring parts of our palæozoic nomenclature are grounded.

From the same combined principles of evidence, I have given (in the following corrected tabular view) what I believe a true geographical nomenclature of the oldest palæozoic groups. It may not be the most scientific (and may hereafter be superseded by something better); but it is a nomenclature, I believe, best suited to the present condition of our knowledge; and it exactly falls in with all those groups which have, on the same principles, been well established in the 'Silurian System.' In every case where this kind of nomenclature has led to a geographical incongruity, we may rest assured that such incongruity is not the fault of nature's workmanship, but has sprung from some misapplication of our principles of classification, or some great mistake in our assumed position of the physical groups.

A Tabular View of the Primary or Palæozoic System of England and Wales, separated into three principal divisions,—Lower, Middle, and Upper.

I. *Lower Division,* representing the Cambrian and Silurian series in ascending groups.

Cambrian Series.

1. Longmynd and Bangor group (Lower Cambrian)
   - *a.* Longmynd slates, &c.*
   - *b.* Llanberris slates; alternations of roofing-slates and grits.
   - *c.* Harlech grits; sometimes approaching a conglomerate form.

2. Festiniog group.... (Middle Cambrian)
   - *a.* Lingula flags.
   - *b.* Tremadoc slates.
   - *c.* Arenig slates and porphyries, &c. Slates, flags, and grits; indefinite alternations of porphyry and trap-shale: one irregular band of limestone near the top of the group.

3. Bala group........ (Upper Cambrian)
   - *a.* Lower Bala rocks. (1) A great zone of dark and sometimes earthy slate. (2) A great series of slates, flags, and grits, ascending to the Bala limestone.
   - *b.* Upper Bala rocks. Under this term are included the Bala and Hirnant limestone, the Llandeilo calcareous flag, and the shelly sandstones of Caer Caradoc. The whole series ending, in North Wales, with slate and flagstone; and, in South Wales, with slates, grits, and coarse conglomerates.

Immediately above these three groups there is a great change of physical conditions. The most characteristic old organic types disappear, and new types take their place. The sections are usually broken and discontinuous; and the upper (or Silurian) groups sometimes overlap the lower (or Cambrian) groups. Here, therefore (to adopt a language in common use), we have the commencement of a new *system.*

* The Longmynd slates are here arranged with the Bangor group, partly on the authority of the Government Survey. The other Cambrian groups are exclusively from my own observations. In a letter, published as an Appendix to a Guide to the Lakes (Hudson, Kendal, June 1853), I have used the following words:—" After the granitic rocks of Wales, we have Metamorphic and Hypozoic rocks. The Metamorphic, of a great, but somewhat doubtful age, and of great thickness. The Hypozoic rocks (of Longmynd, &c.) of very great thickness, and also of doubtful age, but probably to be linked to the lowest Cambrian groups, and to be placed on the general parallel of the Skiddaw slates. If so, they will cease to be Hypozoic, and may then be considered as the lowest known base of the Cambrian series.

Lower Division continued.

Silurian Series.	4. Wenlock group	*a.* May Hill sandstone and Pentamerus, or Norbury limestone. *b.* Woolhope (or Lower Wenlock) limestone. *c.* Wenlock shale. *d.* Upper (or great) Wenlock limestone.
	5. Ludlow group	*a.* Lower Ludlow rock. *b.* Aymestry limestone. *c.* Upper Ludlow rock. *d.* Tilestone.

The introduction of the May Hill sandstone as a part of the Wenlock group is the only change I have made in this portion of the 'Tabular View,' which is prefixed to the 'Second Fasciculus of the Cambridge Palæozoic Fossils.' It gives a true physical and palæontological base to the Silurian series; and assuredly there is not (under this arrangement) any such thing in nature as a "Middle Silurian Group," which inseparably links together the Cambrian and Silurian series, and makes them into one system.

II. *Middle Palæozoic Division.*

(*Devonian Series or Old Red Sandstone.*)

In Herefordshire and South Wales.

1. Cornstone group.................... *Cephalaspis,* &c.
2. Red sandstone and conglomerate .... *Holoptychius,* &c.

In Devonshire and Cornwall.

1. Liskeard and Ashburton group.

2. Plymouth group ....
{ *a.* Great Devon limestone.
{ *b.* Calcareous slates.
{ *c.* Coarse red sandstone and flagstone.

3. Dartmouth slate group......
{ Coarse roofing slates, ending in North
{ Devon, with beds of red, green, and
{ variegated sandstone.

4. Petherwin group ....
{ *a.* Marwood sandstone.
{ *b.* Petherwin slate and *Clymenia* limestone.

In Devon and Cornwall the above series has no base; and we are without any good evidence as to the beds below the lowest Devonian group. Hence there is much uncertainty as to the exact coordination of the series of Herefordshire with that of Cornwall; for the Herefordshire cornstone has characteristic fishes without characteristic shells and corals; while the Devonshire and Cornish series has characteristic shells and corals, but is without fishes.

In a former scheme * the Liskeard and Plymouth groups were united; but they may, I think, be conveniently separated.

* Advertisement to the Second Fasciculus of the Cambridge Palæozoic Fossils.

The Herefordshire fish-beds (cornstone) I had formerly placed over the Plymouth group, on the understanding that certain supposed fish-beds of Cornwall were probably of the age of the Dartmouth group.   As, however, the Cornish fish-beds have disappeared from the sections, I should think it much the safest to place the cornstone group at the base of all the groups of Devonshire; especially as it seems in some places to pass downwards into the tilestone of the Ludlow group, and therefore *appears to give us* (what we do not find in Devon and Cornwall) a base to the Devonian series.   In this way we might arrange the several groups in a regular numerical order, and unite them numerically to the Silurian series.   But the phænomena in Scotland seem to forbid any such intimate union.

The Petherwin group is *provisionally* arranged in the Devonian series.   Physically, it is best connected with the older groups of Devonshire and Cornwall; but *palæontologically*, it is as well connected with the rocks of the Upper Palæozoic division *.

Devonian Series of Scotland.

Grand as is the development of the old red sandstone of Herefordshire, it dwindles into insignificance when compared with the rocks which pass under the same name in Scotland.   They are divided by Miller as follows :—

    1. Great conglomerate and red sandstone.
    2. Bituminous schists—*Dipterus, Pterichthys, Coccosteus,* &c.
    3. Red and variegated sandstone.
       [These three groups form the " lower formation," as well seen in Caithness.
    4. Gray sandstone, earthy slates, &c. [This " middle formation " contains a peculiar group of fishes, *Cephalaspis,* &c.]
    5. Red sandstone and conglomerate.
    6. Impure concretionary limestone.
    7. Yellow siliceous sandstone.
       [The last three, called the " upper formation," are characterized by *Holoptychius,* &c.]

This vast North-British series has no true palæontological base; but its " upper formation " seems to graduate into the carboniferous series.

Its lower groups have no known representatives in the old red sandstone of Herefordshire, and cannot be drawn into comparison with the " *Tilestone*" and fish-beds of the *Ludlow rock.* But its middle group and "upper formation " are represented, though imperfectly, by the " cornstone" and overlying conglomerates, &c. of Herefordshire.

From the above facts it seems to follow,—(1) that the old red sandstone of Herefordshire and South Wales (spite of its

    * This question is discussed in a paper on the Slate Rocks of Devon and Cornwall, Quarterly Journal of Geol. Soc. vol. viii. 1852.

apparent passage into the Tilestone) is defective at its base ; (2) that on British evidence (the only evidence here considered) we cannot unite together the great groups of Devonshire, Herefordshire, and Scotland, so as to form one connected and unbroken series, to be linked, in ascending numerical order, to the upper groups of the Lower Palæozoic division.

III. *Upper Palæozoic Division.*

Carboniferous Series.

1. Coal-field of the basin of the Tweed, and lower coal-field of Scotland. [Under this group may, perhaps, be finally arranged the " carboniferous slates " of Ireland, and, perhaps, the Marwood and Petherwin groups above mentioned. The coal-field of the Tweed appears to include the " great scar-limestone " of Derbyshire and Yorkshire.]
2. " Limestone shale " of Derbyshire, " Yordale series " of Phillips.
3. Mill-stone grit.
4. Great upper coal-field of England, to be divided into two or three sub-groups.

Upper Division continued.

Permian Series.

5. Coarse red sandstone and conglomerate, generally unconformable to the carboniferous strata. It contains (though rarely) true carboniferous fossils (*Lepidodendra, Stigmariæ*, &c.), which may, perhaps, have been drifted mechanically out of the contiguous coal-fields into this coarse, overlying Permian sandstone.
6. Marl-slate and their bedded compact limestone; a few impressions of plants; shells of palæozoic genera—*Producta, Spirifer*, &c.; many impressions of fishes — *Palæoniscus, Platysomus, Pygopterus, Acrolepis*, &c.
7. Magnesian limestone, in some parts of the north of England of great thickness, and most complicated structure : *e. g.* rarely a crystalline dolomite, compact, cellular, earthy, brecciated, globular, oolitic, &c., occasionally with organic remains—*Producta, Spirifer, Synocladia, Fenestella*, &c.
8. Red gypseous marls, very slightly saliferous.
9. Thin-bedded gray limestone, sometimes cellular and dolomitic. A few traces of bivalves, &c.
10. Red gypseous marls. [The above series is overlaid by the great red and variegated sandstone which forms the base of the *Trias*.]

The preceding ten groups are derived from the sections of Yorkshire and Durham, where the series is best developed *.

In the south of England the whole series is sometimes represented by a mass of conglomerate. In central England, Warwickshire, &c. it is represented by a coarse red sandstone, some beds of which become calcareous; and the whole group is con-

* It is evident from the description of these groups, as well as from their general want of conformity to the carboniferous groups, that the Permian series of England is *physically* more nearly connected with the triassic than with the palæozoic rocks. But its fossils are of a decided palæozoic type. If we adopt the term Permian as a general designation of the series, it must be done with proper limitations derived from the English types. For to class under the palæozoic name, Permian, the red sandstone of St. Bee's Head, or the great red sandstone of central England, would, I think, be perfectly erroneous.

formable to, and appears to pass into, the coal-measures. About the commencement of the Triassic period, these Permian sandstones underwent contortions along with the coal strata; in consequence of which we see, in Warwickshire, the upper triassic groups resting discordantly upon the inclined beds of the sandstones.

In conclusion we may remark,—

1. That it is not in all cases an easy matter to draw a clear line between this series and the Carboniferous. Thus Mr. W. Smith, in his old geological map of Yorkshire, considers the lowest group (No. 5) as one of the coal-measures. The series most frequently commences with a discordancy of position and a coordinate change of organic types. With limited exceptions, the flora and fauna of the Permian groups differ from those of the carboniferous period.

2. The several groups of the series admit of very close comparison with the Rothe-todte-liegende, the Kupfer Schiefer, and the Zechstein, &c. of Germany.

As a general conclusion to the preceding tabular view, we may remark, that in the vast succession of the palæozoic rocks there is a difficulty in drawing a well-defined line between the several subdivisions—Cambrian, Silurian, Devonian, Carboniferous, and Permian. But that difficulty is no reason for confounding any two of the subdivisions under one name. To do so would be to conceal a difficulty, not to solve it. There ought now to be no difficulty in the separation of the Cambrian and Silurian groups; for a clear line has been drawn between them. But there is as yet an unsolved difficulty in drawing a line between the true Silurian and the Devonian groups; and there yet remains a doubt as to the exact line of separation between certain Devonian and carboniferous rocks, as well as between some carboniferous beds and the base of the Permian series.

Conclusion.

Returning to the Lower Palæozoic divisions. On what grounds, it may be asked, have I endeavoured to fix their geographical nomenclature? On the same grounds (I reply) on which the Silurian and Tertiary nomenclature (and that of all other secure parts of the whole English series) have been fixed—namely, by determining by hard field-labour the real succession of physical groups; and then completing them, and sometimes correcting their boundaries, by the help of their organic contents. I affirm, with perfect confidence, that the Cambrian series is rightly named, because its successive subordinate parts have been established on that double system of physical and fossil evidence to which I have before alluded. The nomen-

clature is right, because it has the unquestionable right of priority,—because it is geographically true—and because it is also palæontologically true. On the contrary, the scheme by which it has been opposed is an innovation, without being an improvement derived from progressive evidence. It has been vindicated only by a direct desertion of the system of double evidence on which the true Silurian groups were firmly established. Hence it is geographically untrue, and it is palæontologically erroneous*.

So long as geologists believed in the existence of a so-called "Middle Silurian Group," there was unquestionably a great palæontological difficulty; and there was a show of reason for affirming that all the rocks of the Lower Palæozoic division were of one palæozoic system, and must therefore have one name, and might therefore be called "Silurian." But we have no right to mask a difficulty by a mere name. If the difficulty had existed (though, in point of fact, the very difficulty arose out of a palpable mistake), it would not have proved the Cambrian groups to be Silurian, but it would have proved that the Silurian groups had no base; and therefore, from the first, had been erroneously described as a *system*. If we rank all the Silurian groups as one *Silurian system*, we establish our upper groups fairly, upon a double scheme of evidence, derived from true physical groups and their corresponding groups of fossils. But we cannot carry the scheme downwards, below the May Hill sandstone, without a positive desertion of our former scheme of evidence. No wonder that a nomenclature derived from such inconsistency of evidence and change of principle, should lead to a glaring geographical incongruity. Its introduction ought never to have been attempted.

But, after all, the supposed difficulty does not exist. Strike the May Hill sandstone from this so-called Middle Silurian group, and we at length obtain a true physical and palæontological base for the Silurian groups. Why it remained so long undiscovered I do not inquire; but the fact does prove the great hindrance to good classification and real progress which arises out of the premature adoption of definite scientific names.

* In his last published scheme of nomenclature, Sir R. I. Murchison classes all the stratified rocks of Wales which are below the Wenlock shale, under the name "Llandeilo formation;" which appears to me nothing less than a downright *reductio ad absurdum*. There is, in the Llandeilo country, no type, or semblance of a type, of the older palæozoic groups of the Cambrian Mountains. What is the Llandeilo flag? Certainly a remarkable fossiliferous group (about the age of the Bala limestone), of which the geological place was entirely mistaken in the "published" sections of the 'Silurian System.' As a type for palæozoic nomenclature it is, therefore, worse than nothing.

Nor is this all. The sections of Caer Caradoc and Llandeilo were the assumed typical sections from which all the " Lower Silurian " nomenclature was derived. But the interpretation of both these sections was (as has been already shown) erroneous; and I may securely state, that neither in the letter-press nor the illustrations of the ' Silurian System ' is there one paragraph or section which gives us a true notion of the geological place of the groups of Caer Caradoc and Llandeilo. Again, I may state that we have no right to mask a positive error by blotting out our base, and by colouring all the older rocks of Cambria as Lower Silurian. If the Lower Silurian groups were out of place, no good nomenclature could be founded on them. Good nomenclature can only rest on phænomena which are fixed and well-defined. If all the rocks below the May Hill sandstone be called Lower Silurian, no doubt the Caradoc and Llandeilo rocks will remain Lower Silurian. But how do we gain this supposed advantage? By as strange an abuse of logic as is to be found in the history of English geology; by a supposed development of two misinterpreted groups; by making them the typical representatives of the great Cambrian series; by a desertion of the principles from which all our good nomenclature had previously been derived, and by throwing our true and intelligible geographical names into an inextricable confusion. Whatever scheme of nomenclature we adopt, let it be used consistently. We cannot with impunity use two distinct schemes of nomenclature, while we are professing to describe but one geological system.

Should any one object to my use of the word *system* as descriptive of our great geological divisions, such as Primary and Secondary, I should on that account have no quarrel with him, for our difference would be one of taste, perhaps, rather than of principle, and could lead to no misunderstanding. There is no difficulty in finding consistent general terms for the great geological divisions; and any one who prefers that use of the word *system*, may in the foregoing Tabular View write *system* in the place of *series*, and it will then give the whole succession under the names Cambrian System, Silurian System, Devonian System, &c. And I may conclude this comment on the Tabular View, by stating (as I have done in a letter " On the Geology of the Lake District *,") that I can still adopt, almost word for word, the expressions published by myself twelve years since; and can affirm with truth that the two great divisions (Cambrian and Silurian) differ in structure, interchange comparatively few fossil species, and through large districts are unconformable. " Hence they belong to two systems and not one, if the word *system* be

* Published by Hudson, Kendal (June 23, 1853).

used in a definite sense, and be applied to the successive *divisions* of the palæozoic rocks, such as the Devonian." (Proceedings of the Geological Society, vol. iv. p. 224.)

Let no one suppose that I undervalue the importance of the great fossil lists derived from the Caradoc and Llandeilo groups. Their *palæontological* value remains unchanged, whatever be the nomenclature and ultimate arrangement of these groups. But I may add, that such fossils have comparatively small *geological value* before their real sectional position (in the general sequence of our palæozoic rocks) has been determined; and, until that point has been cleared, they are of *no value* in helping us to a true nomenclature of our palæozoic groups. Many years before I ever set my foot in Wales, and before the ' Silurian System' was thought of, the *Asaphus Buchii* and other species were known as characteristic of the flags of Builth and Llandeilo. Some of the species (then unnamed) from Snowdon, Meifod, Coniston, &c. were not unfamiliar to collectors; and very beautiful collections from the Silurian groups (especially the Wenlock) had been made, and some of the species had been published. But this knowledge, even had it been ten times greater than it was, could give no help to the advancement of palæozoic geology, until the position of the fossil-bearing groups had been approximately determined. So far as this task was done in Siluria, the ' Silurian System,' and its geographical nomenclature, stands good; and in the preceding Tabular View the author of that " System " is not deprived of one single group of which the true place was determined in his published sections.

If all the colours of the Silurian Map were laid down in agreement with the natural scheme above given, there would be but a comparatively small deviation from the original and first published colours of the map, with one partial change of name—from *Caradoc sandstone* to *May Hill sandstone*. The Silurian Map of the Usk, of Tortworth, of May Hill and Woolhope, of Presteign, of the Malverns and Abberley, &c., would, with one almost evanescent exception, remain unchanged. The Caradoc and Llandeilo colours must go out from the map; but the loss will be amply compensated by a great extension of the May Hill colour over many tracts that were coloured Cambrian in the original Silurian Map, through the chain of the Berwyns, and in a range of hills from the neighbourhood of Corwen to Conway.

What I stated to the Geological Society (in my paper of February 25, 1852) as an hypothesis, some one might think too partial to my own views, I can now state with more confidence on the authority of one of the greatest of the living geologists of America. His words are as follows :—" I am satisfied that the evidence from fossils entirely sustains your remarks—that

the development of animal types from the early dawn of a living world, appears to have been carried on in North America in strict analogy with the development exhibited in the British Isles; and your just inference, that the scheme of development in your Tabular View will be more acceptable to the American geologists than any other scheme of arrangement of the British rocks which has yet been published *."

Before I conclude, I may remark, that if some pages of this paper be controversial, they are brought forward with no petulance, but with a truth-loving spirit, and in a conviction of their importance. It is impossible that observers should agree during the investigations of a continually advancing science; and earnest respectful discussions on unsettled points of nomenclature or classification of facts, so far from being unfit for the consideration of a body like the Geological Society, are, under proper regulations, of all subjects perhaps the best fitted for their sober discussion. To adopt a rule that would prevent the mooting of controverted questions, would be most unjust to the cause of scientific truth and scientific progress. It would virtually assume, that whatever scheme was once published in its memoirs must never afterwards be the subject of criticism or of change— a principle which could not be followed out by any Society without very soon being injurious to its honour, or fatal to its existence.

XLVII. *Some account of the Discoveries of* Keber *on the Porosity of Bodies; with Confirmations.* By MARTIN BARRY, M.D., F.R.S.

[Concluded from p. 293.]

1. *Method of proceeding with Dry Objects.*

" *a.* WITH a fine hair pencil I first carefully cleansed the object to be examined, the scalpel to be used, the glass on which the object to be examined was to lie, as well as the covering-glass, removing all adherent dust, in order thus to prevent any confounding with other particles; then, holding the object to be examined over the glass, I *very gently* scraped with the scalpel upon its surface, spread out the minute scrapings (partly fallen and partly hanging to the knife); which was done either with fine needles, or, still better (in order to prevent a sticking together of the scrapings), by *tapping,* or gentle strokes with the blade of the knife upon the glass. I then examined them *without moistening,* but with a covering of glass under the aplanatic eye-piece of the compound microscope with 200

* Extract from a letter of Professor Henry Rogers, of the United States.

diameters linear and full transmitted light. The microscope stood 4 to 6 feet from the window. The covering-glass was employed in order to prevent any confounding with other particles that might possibly fall upon the glass during the examination. Of the scales and lamellæ in the field of view I then sought out the *smallest*. Those scales which, from their minuteness, are scarcely or not at all visible with the naked eye, are best adapted for perception of the pores. There is seen a delicate net and lattice-work of variously interlaced fibres, with lamellæ more or less covering one another, which, however, partly between them, partly in their substance itself, present a multitude of minute, irregularly shaped, roundish, elongated, indented, and angular orifices, spaces, or rifts, which are sometimes dendritically branched, and form a system of communicating hollows, interstices, or passages.

b. The making of minute scrapings of dried animal membranes and other animal formations, for example, bones, enamel of the teeth, egg-shell, horn, &c., as well as of soft metals, such as gold, silver, lead, is not difficult; but with very hard bodies, such as granite, iron, and many crystals, it is not to be accomplished, because by their hardness the knife is too much laid hold of, and the detritus hence arising might easily be confounded with the scrapings of the formations intended to be examined. In examining the harder bodies, therefore, I have thought it fitting to use two substances of the same kind, for example, two pieces of steel, granite, &c., which, held over a glass from which the dust had been wiped off, were gently rubbed the one upon the other. The detritus, either falling from or cleaving to the substance, was then covered with glass for examination.

In both ways I succeeded with certainty in recognizing in all solid bodies, not merely at their surface, but through the whole thickness of their substance, and in all their parts, sometimes marks of general porosity, sometimes even individual pores; and very interesting was it to notice a surprising concordance between the disposition of their parts and that of their minuter structure. Thus if the attention be directed to the smallest particles of the detritus so obtained, the addition of water being avoided, there may be perceived in all bodies the minute disposition and arrangement of delicate granules, filaments, scales, and lamellæ; producing the conviction that the expression *structureless*, now-a-days so great a favourite in histology, can have a title to be used only in so far as it is intended to denote the condition hitherto of our knowledge, namely, the absence of (what is already known and has been erected in the system under fixed names) the morphological elements of texture. *The smallest atom is not really structureless!* The microscopic

examination of all objects in the condition of minute division, according to the above simple methods, teaches this. Through this discovery, not difficult to test, the representation of the composition of all organic structures out of the finest fibres (the latter again representing a higher stage in development of the nucleolus), given long since by Martin Barry[*], receives its full confirmation and extension ; and it thence follows that the organic cell, which is regarded by many in the present day as an "elemental structure," and in the cell-membrane is said to possess a "structureless" covering, already occupies a high step in the series of natural bodies.   It is hoped that every one of those who may determine to examine it in a minutely divided state, and without the addition of water, under a superior microscope, will be obliged to admit that the cell-membrane is far from being structureless.

The addition of water, generally speaking, is a hindrance to distinct perception of the microscopic pores, for sometimes the textures—namely, in organic bodies—are thereby made to swell, sometimes owing to altered refraction from passing through pores filled with water, the rays of light no longer show with the same distinctness the above-mentioned striking light-reddish colour ; yet, with long practice and great intimacy with the subject, porosity, or at least traces of it, may be recognized even though water has been added.

c. Another method for finding microscopic pores in all dry natural bodies, just as simple as that now mentioned, yet taking up more time, is the following :—Carefully remove all dust, as well from the body to be examined as from the glass on which its particles are to lie.   Then, upon one fixed spot of the glass, make *gentle but continued* tappings with the body.   By this means there are usually loosed therefrom extremely delicate spangles [*Flitterchen*], porous quite through their substance. To prevent any confounding with dust-particles, it is needful with the utmost care first to cleanse completely the receiving glass, and even to employ the microscope in order to see that that part of it upon which the objects to be examined are to fall is completely cleansed.   This method, it is true, takes more time than that first mentioned, but it is very sure.   It is also easy of application, and this not only to dried animal membranes (which for this purpose should be folded into a cone-like form), and to all vegetable formations, but also to soft metals.   Further, on account of the great delicacy of the detritus hereby obtained, it is most rewarding.   For example, if one with forceps lays hold of a little bit of gold-leaf and taps therewith gently upon a fixed spot of the glass, there are loosened from the gold-leaf

[*] Phil. Trans., 1842.

extremely delicate spangles, which show the same porosity as the finest metallic scrapings obtained by the first-mentioned method. Now as the particles loosened by gentle tapping perfectly correspond in their minutest structure with that of the detritus procured by scraping, they serve at the same time as a proof that the porosity of the finest scrapings is by no means artificial, effected through the operation of scraping, but that it is a natural formation. Further, through the tapping method porous spangles are loosened, not only from the surface of the object, but also (if one cuts it up into points and separates the latter) from its *inner* parts, and *throughout its whole substance,*—spangles, the minutest structure of which throughout corresponds with that above described. It follows hence, that we have here to do with the natural structure of the body itself, and not with an artificial product arisen from mechanical friction and continued tapping, or through the weather.

There is another advantage in the tapping method. By means of it one is able to perceive with more certainty and ease the structure of all *vegetable* structures, than by means of scraping; for as the vegetable structure, generally speaking, is denser and more compact than the structure of animal formations, it is incomparably more difficult in the former to prepare scrapings fine enough for distinct recognition of the pores. Hence it is well for us to have the method of gentle tapping to resort to.

2. *Method of proceeding with Bodies that are fresh and still moist.*

In animal and vegetable bodies that are fresh and still saturated with moisture, may be recognized as well porosity in general as also individual pores. In order to this, pass the knife most gently over their surface, lay the detritus upon glass, and examine *without* the addition of any water and *without* a covering of glass. By the latter, a covering of glass, the microscopic pores in moist organic substances are not only more or less pressed together, which renders a recognition of them more difficult, but evaporation of the plasma which saturates the texture is retarded and prevented. Now such evaporation being just what mainly contributes towards a recognition of porosity, the more distinctly does the latter appear *the further the evaporation has proceeded under the eyes of the observer*. In the examination of dried substances these collateral effects of a glass covering are not to be apprehended, as the former sometimes no longer contain any fluids, and sometimes cannot be so easily compressed; and its application here is usually indispensable in order, as already said, to prevent any confounding with dust-particles which may possibly fall upon the glass during the observation.

Now this circumstance, that, in thus proceeding, porosity is perceptible in entirely fresh organic formations too, immediately meets the objection which perhaps might be taken—that this porosity is merely an artificial product, the effect of drying. I would here however bring to especial remembrance the fact, that, as experience shows, organic formations by no means lose their natural texture through mere drying; for dried animal and vegetable textures, through fresh moistening, reassume their former appearance and original condition *.

Very conspicuous are the pores in the roots of plants when examined after proceeding in the following manner. Take fresh roots filled with their juices or after having lain in water,—remove the earth,—apply gentle scraping, and then spread out upon glass for examination, without a covering of glass, the detritus adhering to the knife. Then, as the fine scrapings on the glass dry up and the moisture contained in their interstices evaporates under the eyes of the observer, the pores in their substance, with intense illumination, are brought out in so distinct a manner that the whole scrapings appear composed of a network of variously twisted and overlapping filaments and scales, leaflets and granules; and in fact everywhere present a composition like that of sponge. The addition of water having been avoided, there is besides observable in the neighbourhood of the scrapings some detritus still much finer than the rest, in which one may very well recognize *the disposition of the granular materials to form minute filaments, and the interstices thereby arising between them.* It requires however a good light and a trustworthy microscope to recognize these pores of the roots of plants; yet a linear enlargement of from 200 to 300 is always sufficient, and from the greater clearness thereby effected, is preferable to higher magnifying powers.

Having now, according to the above-mentioned methods, examined with perseverance and care all the animal and vegetable formations of many classes in both kingdoms which it was possible to get access to, as well as all sorts of inorganic bodies, I consider myself entitled to make the enunciation, that now-a-days porosity in all solid bodies admits of optical demonstration. Far, however, am I from maintaining that every light-reddish glimmering place in fine scrapings is always an open space; rather do I unconditionally admit that many of them are covered by other delicate lamellæ. Yet in my opinion this circumstance proves nothing against the porosity of bodies generally, which is more required to be established than the perception and measurement of individual pores : though during an occupation

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* J. Liebig, *l. c.* p. 2.

with this subject that lasted many months, I met with no scrapings in which unequivocal pores were entirely missed.

But with respect to the objections which many will feel inclined to aim at the correctness of my statements, they have been mentioned briefly in the foregoing, and amply treated of and met in my paper; so that to avoid being too diffuse, I must refer thereto. As for the rest, I am convinced that practised observers, should they determine with their own eyes intimately to test the accuracy of my statements according to the above described methods, will be directly led to the firm conviction that the porous structure of the particles is not the product of mechanical division into minute parts, but a natural formation, and that the porosity perceptible in the same is no optical illusion.

Among the organic and inorganic bodies examined by me according to the above methods, I would especially mention the shell and membranes of the egg, the epidermis and cutis of man and many animals of different classes; further, horn formations, hair, the cell-membrane, the mucous and vascular membranes, the walls of capillaries, lymphatics, blood-corpuscles, serous membranes, ligaments, bones, and teeth. Further, my examinations have extended to all parts of plants, in the course of which I repeatedly discerned the microscopic pores most definitely in the roots. In my paper already mentioned, plate 1. figs. 6, 7, 8, represent the microscopic pores of the outer layer of a white bean, of a potato-peeling, and of cork. Lastly, of the vegetable formations examined, I have to mention charcoal, pit coal, and brown coal. Of the inorganic bodies whose microscopic pores I have found, I would make especial mention of gold, tin, silver, lead, iron, granite, many crystals, &c. My paper, in plate 1. figs. 10, 11, contains drawings of the microscopic pores of gold and of iron. The pores of granite measure $\frac{1}{1200}'''$ (*i. e.* Paris line) in diameter, those of iron $\frac{1}{2000}'''$ to $\frac{1}{3000}'''$; those of steel, which however are very difficult of demonstration, appear to be still somewhat smaller. The average size of the pores in all vegetable formations may be taken at $\frac{1}{1300}'''$, among which there occur individual variations of from $\frac{1}{1000}'''$ to $\frac{1}{2000}'''$. The pores in animal formations as to size do not widely differ from those of plants; they appear however to be a trifle larger. In the shell of the hen's egg they measured $\frac{1}{800}'''$ to $\frac{1}{2000}'''$, in the membrana testæ on the average $\frac{1}{1200}'''$, in the membranes of the ovum of Man and the Rabbit $\frac{1}{1200}'''$ to $\frac{1}{1600}'''$, in the human cuticle and skin the same. The pores of the epithelium and mucous membranes had the same size, while in the epithelium and all other layers of the vessels I remarked and measured pores from $\frac{1}{1200}'''$ to $\frac{1}{2000}'''$, in individual cases however even large rifts or clefts from $\frac{1}{600}'''$ to $\frac{1}{800}'''$. The pores of the lym-

phatics measured $\frac{1}{1600}'''$, those of the serous did not differ from those of the mucous membranes; the pores of the bones and teeth in cases which I examined, measured from $\frac{1}{1000}'''$ to $\frac{1}{1600}'''$. In respect of all other details I once more refer to my repeatedly mentioned paper.

The physiological and pathological conclusions following the above mentioned facts require, for the reader acquainted with his subject, no more than a short intimation. As from the results of my examinations the vascular membranes are *not* "hermetically closed," but porous quite through,—as even the walls of the capillaries are not "structureless," but consist of a network of the finest filaments and plates, between which the microscopically perceptible and measurable spaces form a *system of the finest hollows and interstices*,—the physiological processes of endosmosis, exosmosis, absorption, nutrition, and secretion, now appear to us as a *natural* consequence of this physiological and mechanical constitution of the vascular membranes. It consequently follows from the above discovery—which, proceeding by the methods I have given is not difficult to confirm— that in all organic textures there are present and optically demonstrable those microscopic openings, without which the most important vital processes must always appear incomprehensible to the natural understanding. By this demonstration there falls one of the most important bounds through which the doctrine of a change of material had so much of the tottering and hypothetical, and a long and on-many-sides sought positive foundation for the understanding of most organic processes has now been shown to admit of objective demonstration.

That the cell-membrane of plants is by no means structureless, but is composed of the finest filaments, has very recently been demonstrated by Agardh[*], who has thereby confirmed the representation given by Martin Barry in 1842.

Further, I would call to remembrance the fact, that through the objective demonstration of the microscopic pores already formed in all animal membranes, the penetration recently shown by G. Newport[†] of the spermatozoa into the frog's ovum seems to find its natural explanation, seeing that the spermatozoa, possessing no perforating organs at their fore-end, are incapable of boring through a poreless membrane; whereas, with the presence of preformed pores, even a *yielding together of the texture-elements* and an enlargement of the natural pores through

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[*] *De cellula vegetabili fibrillis tenuissimis contexta,* Lundæ, 1852, page 3.—See Proceedings of the Royal Society of London, March 17, 1853.

[†] Phil. Trans. 1853, p. 271, Note.—The penetration of the spermatozoa into the Rabbit's ovum had been discovered by Martin Barry ten years before. (Phil. Trans. 1843.)

mechanical pressure may follow. Here too belong the penetration of entozoa and their ova into the blood,—the penetration of molecules of mercury, fat-globules, even finely pulverized solid bodies, through the pores of the epidermis and of the mucous membranes,—the passing out of blood-globules through the loosened capillary walls in menstruation,—the pressing through of blood-corpuscles in bloody secretions, in pneumonia, &c.,—the penetration of the materials of disease into the blood through the outer skin and the mucous membranes,—the finding in the blood of substances introduced, &c.

With regard to vegetable physiology, and in respect of the experiments long since instituted by Stephen Hales and very recently confirmed by Liebig*, I must give utterance to the declaration, that since my discovery of the visible and measurable microscopic pores in all vegetable formations, the currents therein may with ease be referred to the physical agencies of capillary attraction, evaporation, &c., without requiring with Dutrochet† to have recourse to an active mysterious *vis a tergo* to explain the rising of the sap. In this manner of viewing the vegetable organism, it appears to us an exquisitely constructed machine indeed, yet set in motion chiefly by means of simple physical forces,—a machine whose universally-diffused porosity permits as well the ascent of the fluids out of the earth surrounding the roots, as also the evaporation of the plasma saturating all vegetable formations, according to physical laws.

I have the additional remark to make, that my paper in question, plate 1. fig. 5, contains a drawing I made, faithfully delineating nature, the result of numerous observations, of the *Aspidogaster conchicola* (Baer) living in the pericardium of *Unio* and *Anodonta*, in which may be observed the pressing through of the granular molecules out of the intestinal cavity into the substance of the body; a process that may be not only observed, but followed through all its stages, *during life.*"

Confirmations.

In testing the accuracy of Keber's observations, so far as my health permitted, I used a $\frac{1}{8}$th-inch object-glass, very lately made for me by Smith and Beck. Of the performance of this object-glass, I need say no more than that the order for it was given by my esteemed friend J. J. Lister,—that it was submitted for his approval before the fitting on,—and that he wrote to me concerning it:—"I can report with much satisfaction of its quality—its correction and defining power being all I should

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* *l. c.* p. 72, &c.
† *L'agent immédiat du mouvement vital,* 1826, p. 90.

expect in a glass of its focal length, with its large aperture (of from 100 to 110 degrees, according to the absence or the thickness of a medium covering the object)." My microscope has a "slow motion" for fine adjustment of focus, given by a nut that has its margin divided into tenths *.

Keber insists on the absolute necessity of obtaining for examination particles that are exceedingly *minute*. He conceived the beautiful idea of first dispensing with art in getting these, and collecting such as are "weathered" off—particles of Nature's own preparing. Not until these had been well studied did he venture to examine the detritus artificially obtained, and of this, too, he always selected the minutest. Nothing could be truer than what Keber says on this subject of *minuteness*. It alone would have been a boon to physiology. I tried his plan, examined particles "weathered" off,—compared these with the minutest of such as had been obtained artificially,—saw the same in both,—and am therefore enabled to confirm his statement that the appearances described by him are not the product of artificial dislocation.

He points out where it is important to avoid adding water, and where to add or not to add a covering of glass. Here, too, my own experience enables me to attest the advantage of rigorously attending to what Keber recommends.

Most aptly does he describe the fine scrapings of the roots of plants, after evaporation has proceeded for a while, as appearing "composed of a network of variously twisted and overlapping filaments and scales, leaflets and granules ; and in fact as everywhere presenting a composition like that of sponge." "A system of communicating hollows, interstices, or passages," as Keber describes what he saw in the detritus of scraped, dry, organic objects, no careful observer can, I think, deny. Such a composition of scales that overlap each other, and fibres that run in all directions, cannot but resemble sponge.

The scales, however, met with among the weathered-off particles, as well as those found when the detritus has been obtained, by scraping or by tapping, in its minutest form, require an additional remark. Each of these scales I find to be *a flat or discoid nucleus* that has divided into many still adherent parts, which are pressed into various forms, each part being itself a nucleus, and having its single nucleolus. The round orifices of Keber, I think, must be the *nucleoli* now referred to.

In animal and vegetable formations, look where you will, you

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* It is known that the best performance of an object-glass of such an aperture as that in question is thus obtained *by measuring the thickness of the medium covering the object*, and setting the collar of the object-glass accordingly.

find either fibres or scales, or both; the scales composed of nucleolated nuclei, and these being the elements of fibre which is sometimes seen formed within the scales.

Mammiferous Red Blood-corpuscles of extreme minuteness, each bearing a Cilium (as described in the Phil. Trans. *for 1841, pp. 245, 246).*

There was a reason why these discoveries of Keber on the porosity of bodies should have especial interest for me. Since 1840, I had been stating facts which showed the material for the elements of tissues, for nutrition, and for the formation of new parts, to be derived from the *corpuscles* of the blood*. I had also been endeavouring to point out the importance of *nucleal self-division.* How satisfactory, therefore, to find as one of the results of researches continued for many months by one of the most careful of observers, that the vascular membranes are " *not* ' hermetically closed,' but porous quite through," and that the capillaries are "not ' structureless,' " but consist of a network of the finest filaments and plates, between which the microscopically perceptible and measurable spaces form a "*system of the finest hollows and interstices,*" sufficient to admit the passage of solid bodies.

Further, how satisfactory now to be able to refer to a description I gave in the Phil. Trans. for 1841, of red blood-corpuscles of extreme minuteness, each bearing a cilium. Regarding these I gave the following particulars. They arise in parent corpuscles. Some of the parent corpuscles are of prodigious size—$\frac{1}{50}'''$. They are always very pale, and sometimes even colourless. You occasionally see them ruptured and partially discharged of their contents. In this state they frequently appear shrivelled. When not ruptured they are filled with young corpuscles. These after liberation acquire red colouring matter. Sometimes they become

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* Phil. Trans. 1840, 1841, Edinburgh New Phil. Journal, Oct. 1847. After closely studying the elements of nearly all the tissues of the animal body, I was enabled in 1841 to write as follows :—" Every structure I have examined arises out of corpuscles having the same appearance as corpuscles of the blood. I may here mention, that the tissues submitted to actual observation, with the result just mentioned, will be found to include the cellular, nervous, and muscular; besides cartilage, the coats of blood-vessels, several membranes, the tables, cells, and cylinders of the epithelium, the pigmentum nigrum, the ciliary processes, the crystalline lens itself, and even the spermatozoon and the ovum. And among the vast number of observations made, 1 have not been able, with the greatest care, to detect a single fact inconsistent with the conclusion above announced. If that conclusion—which regards the *formation* of the tissues—be correct, it may, I think, assist us in considering ' the mode in which the floating corpuscles of the blood conduce to nourishment' *during life.*"—*Phil. Trans.* 1841, p. 217.

tinged with red before their liberation. When liberated they present a star-like form. This form I in some instances noticed them to have while within the parent corpuscle. Before becoming star-like, they present about half-a-dozen segments; each segment sends out a cilium, and hence the star-like form. Such corpuscles are seen slowly revolving, and even performing loco-motion. They are also seen effecting changes in their form—struggling, as it were, in order to separate into parts, each part or segment bearing its cilium. After such separation the ciliated segments exhibit extremely vivid motions. But these are far from being the minutest red corpuscles met with in the same blood. Many are seen not exceeding in size the separated segments just mentioned, but star-like in their form. [May not these arise from the segments in question, which are nuclei, performing self-division?] Lastly, there are other bodies so immeasurably small as to appear as mere points. They have precisely the same red colour as corpuscles of larger size, and exhibit most vivid motions. I believe these to be no other than separated and ciliated segments of the *last*-mentioned generation of star-like corpuscles.

Such then was the account I published at the time mentioned, of this self-division of red corpuscles of mammiferous blood. It may be asked: Where do such self-divisions end? Who will say that exudation corpuscles and all that the blood deposits for nutrition or the formation of new parts, are not nuclei thus derived from corpuscles of the blood?

Where does the formation of cilia cease? Who will say that spermatozoon-like blood-corpuscles, of a minuteness that no magnifying power can reach, do not escape the vessels, *sculled* by cilia through the pores?

XLVIII. *On the Magnitude of Galvanic Polarization.* By W. Beetz*.

THE experiments of Lenz and Saweljew have demonstrated that the polarization of a platinum plate by means of chlorine is nearly equal to zero, whereas I have found that the electromotive force of platinum and chlorine in a gas battery is more than half as great as the force of platinum and hydrogen, and at the same time it was manifest that the effect produced upon a charge already established, by agitating the platinum electrode covered with chlorine, was to increase the current. As the measurements of the Petersburg physicists are vitiated by numerous sources of error, I instituted new experiments, and thereby deter-

mined the charge of two platinum plates between which hydrochloric acid was decomposed. In these experiments either both electrodes consisted of platinum, in which case the polarization Pt(Cl) + Pt(H) had to be subtracted from the original force of the current, or the positive plate consisted of zinc, and was immersed in a solution of sulphate of zinc, when the polarization Pt(H) alone remained; or lastly, the negative plate consisted of copper and was immersed in a solution of sulphate of copper, in which case the polarization was Pt(Cl). In the last two cases the force of a second battery was introduced into the circle; that is, Pt Cl H + ZnO SO³ in the one case, and Cu SO³ + Pt ClH in the other.

These forces were measured by the compensation-method, and then introduced into the calculation with their proper signs. For the sake of convenience, all the electromotive forces which will be employed in the following experiments are collected together in the following table. According to the unit here employed, the force of a Grove's battery was found from a mean of twelve measurements to be 37·26.

I.

Pt SO³ =40·26	Pt K Br =29·50
Pt NO⁵ =36·24	Pt ClH =29·10
Pt SO³ Aq =32·66	Pt KI =21·67
Pt K Cl =31·97	Cu CuO SO³ =21·22
Pt Na Cl =31·80	Zn ZnO SO³ = 1·34
Pt Na Br =30·79	Zn SO³ Aq = 0

These numbers are for the most part deduced immediately from measurement, and partly by employing the law of the electromotive powers of bodies to the actual measurements. At first, the circuit of a battery of platinum and zinc, consisting of one or more elements, was closed by means of two platinum plates in hydrochloric acid. By measuring the force of the battery alone, and its force after introducing the hydrochloric acid, the following six values were successively obtained for the—

II.

Polarization, Pt (Cl) + Pt (H).

29·30
27·91
30·29
30·32
27·45
27·75
Mean . . 28·83

This force varied very little with the increasing intensity of the decomposing current.

Further, platinum was used for the negative, and zinc for the positive electrode; the former being immersed in hydrochloric acid and the latter in dilute sulphuric acid, so that both liquids were in contact with a cylinder of porous clay. The result was,—

III.

Polarization, Pt(H).

19·08
19·23
20·26
17·36
19·48

Mean　.　.　19·08

. In a similar manner, by employing platinum immersed in hydrochloric acid for the positive, and copper immersed in a solution of sulphate of copper for the negative electrode, the following numbers were obtained:—

IV.

Polarization, Pt(Cl).

10·46
9·79
10·34
10·03
10·20
10·78

Mean　.　.　10·27

Hence the—

Polarization by chlorine　= 10·27
...　hydrogen　= 19·08.

Therefore the polarization by both gases is—

Calculated.	Observed.
29·35	28·83

Consequently, the charges have here been added together; *but the absolute magnitudes of these charges are the same as those which I before found for the electromotive forces of chlorine and hydrogen in a gas battery;* for the latter, when reduced to the present unit, would be 10·10 and 17·89*.

That such a near coincidence should take place in the decomposition of hydrochloric acid, and not in that of dilute sulphuric

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* Phil. Mag. S. 3. vol. xxxvi. p. 81.

acid, may probably be thus accounted for; in the former case the whole electrolyte is exactly decomposed into its constituents, whilst in the latter case, besides the decomposition of water, a separation of the sulphuric acid from the water also takes place, consequently a second polarization occurs.

On this account I endeavoured to obtain more of such decompositions, and employed for this purpose the haloid salts of the alkaline metals, from which the halogens were separated at the positive platinum electrode, whilst a plate of copper, immersed in a solution of sulphate of copper, served as the negative electrode. At the commencement I convinced myself that the same polarizations occurred in these decompositions as in those of the hydrogen acids. The following are the results :—

<p style="text-align:center">Polarization, Pt (Cl).</p>

In the decomposition of a solution of—

V.	VI.
Chloride of sodium.	Chloride of potassium.
10·54	11·36
10·90	11·28
10·95	

Mean from V. and VI. $= 11·01$,
Mean from IV., V. and VI. $= 10·58$.

<p style="text-align:center">Polarization, Pt (Br).</p>

In the decomposition of a solution of—

VII.	VIII.
Bromide of potassium.	Bromide of sodium.
7·10	7·11
6·46	6·93
6·86	

Mean from VII. and VIII. $= 6·89$

In the decomposition of a solution of iodide of potassium the following values were obtained :—

<p style="text-align:center">IX.</p>

<p style="text-align:center">Polarization, Pt (I).</p>

<p style="text-align:center">3·22
3·91
3·65
———
Mean . . 3·59</p>

The following table is deduced from the above and from former observations :—

	Polarization.	Electromotive force.
In iodine	3·59	3·36
In bromine . . .	6·89	6·96
In chlorine . . .	10·58	10·10
In hydrogen . . .	19·08	17·89
In chlorine + hydrogen	28·83	27·99

Consequently the law above expressed is true for all these substances.

In a former memoir[*] I found that the polarization of a platinum plate in dilute sulphuric acid, in comparison to the force of a Grove's battery, was as 21 : 32, hence its true value is

$$24\cdot4\,;$$

later measurements gave a mean value equal to

$$25\cdot30,$$

or six units greater than in hydrochloric acid.   This difference may probably be explained by the separation of the water.

The deviation of the polarization Pt(O) (=24·4) from the electromotive force of this gas (=3·42) is, however, very great indeed; it cannot certainly be explained by the concentration of the acid.   I expected to find that the formation of ozone caused this deviation, inasmuch as, according to its chemical and physical properties, ozone always deports itself as negative with respect to chlorine.   It is true that all attempts to generate a sufficient force by converting oxygen into the allotropic condition by means of an electric discharge were unsuccessful; nevertheless I do not relinquish the hypothesis, seeing that, according to this method, only small traces of the allotropic oxygen could arrive at the gas battery, the greatest part being consumed in the formation of ozone, or becoming converted into the usual condition. The polarizing action of the allotropic oxygen, therefore, is limited to the moment of its production; and hence the polarization of the positive electrode may sink in so quick a curve to the electromotive force Pt (O)[†], whilst that of the negative electrode has only to traverse the small fall Pt (H), perhaps only to the reproduction of the original condition of the liquid.

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[*] Pogg. *Ann.* vol. lxxviii. p. 3.
[†] Ibid. vol. lxxix. p. 10.

XLIX. *On the Periodical Variations of Terrestrial Magnetism.* *By* A. Secchi, *Director of the Observatory of the Collegio Romano**.

IN all physical problems it is from the intercomparison of phæ-nomena that great discoveries may be hoped for, and more especially so when the cause of the phænomena appears very ob-scure and complicated. Such are the diurnal and annual variations of the magnetic needle, which, being manifestly connected with the sun's movements, have been attributed to that heavenly body, by supposing it to act either directly as a magnetic body, or indi-rectly through the variations which it produces in terrestrial temperature. Dr. Müller, in two memoirs published in the Italian *Ateneo* (which he has kindly sent to me), has recently examined the correspondence between the magnetic variations and those of temperature, and has proposed new methods for studying it in future. In the analogy found by him, and which appears to me to have been carried too far, he has perhaps been led astray by an imperfect graphical method, and by paucity of materials; those used by him being only the observations of Arago and Quetelet, and of these only such as are quoted by Kaemtz. It does not appear that he has attempted to discuss the gigantic works, which have been executed in recent years in so many and such well-equipped magnetic observatories scattered over the globe, and more especially those established at the ex-pense of the British Government, the observations of which have been published and discussed by Colonel Sabine in nine large volumes, published at the expense of the same Government; for Dr. Müller says, in p. 2 of his memoir, "that in the absence of long-continued observations and well-assured data, the results hitherto obtained are but little satisfactory."

I have thought it useful to supply this omission, and at the same time to complete the very imperfect information given re-specting this branch of knowledge, even in the most esteemed courses of instruction, by a brief exposition of what has been hitherto done and discovered. While thus engaged, there pre-sented itself to my apprehension a law of great simplicity, by means of which all the facts are reduced to a very simple theory. So much simplicity of law, in phænomena apparently so irregular, encouraged me to publish this memoir, which naturally divides itself into three parts :—

1. The historical exposition of what has been done towards the study of the variations of terrestrial magnetism.

2. The systematic exposition of the consequences deduced.

3. The discussion of the hypotheses hitherto propounded for their explanation.

I hope this memoir will not be unacceptable to my readers, as the sources to which I have fortunately access, if not all that are possible, are at least the best, and moreover are such as are not at every one's command.

At the present time, when observations and inquiries of all kinds are so much multiplied, it is besides most desirable that such classical works as have already been performed should be widely known, that time and labour may not be lost by doing again what has been already well done by others.

PART I.—*Brief historical Exposition of what has been under-taken for the study of the Magnetic Variations.*

In the study of great problems in terrestrial physics such as the present, the zeal and activity of a single observer are quite insufficient, not only on account of the multitude of data required, but also because simultaneous observations in many countries are needed; thus the aid of scientific societies, and sometimes even the active cooperation of governments, becomes indispensable. So long ago as 1761, the observations of Cassini in France and Gilpin in England, repeated at Rome by P. Asclepi in 1762, had shown that the needle has a diurnal variation, and the latter had even suspected an annual variation. In 1741, Celsius in Sweden and Graham in London, by a series of concerted corresponding observations, discovered the simultaneity of great magnetic perturbations at different points of the globe; a discovery which was afterwards forgotten, and was remade by the observations, also simultaneous, of Arago in Paris and Kupffer at Kasan in 1825.

The illustrious Baron Alexander von Humboldt undertook in 1806, at first alone and afterwards with the help of others, a series of uninterrupted observations on certain days in the year. They consisted in observing the needle every five minutes on the days of the solstices and equinoxes. The apparatuses employed by these first observers were different,—Humboldt used a magnetic collimator of Prony, and Arago a variation needle of Gambey; but notwithstanding the anomalies which this circumstance was likely to introduce, the simultaneity of the perturbations and the parallelism of the movements of the needle were manifested at Berlin, Paris, and in the Mines of Freyberg 66 metres deep. Between 1832 and 1836, Gauss, having directed his learned researches to the theory of terrestrial magnetism, furnished its students with new methods and instruments of observation, and thus introduced a new æra in the science. There was then organized a society of observers who undertook to make observations

at the same precise instants of time, on particular days, at different places. The magnetic observations at Göttingen became the type of all establishments of this kind, and at all, the observations were made, for the sake of uniformity, in Göttingen time, and with astronomical exactness. The first conclusions thus obtained were very important. Amidst many irregularities very general laws were perceived. The simultaneity of perturbations at places considerably distant was confirmed, as well as the influence of auroras or northern lights, even at a distance, and the influence of seasons and of different hours of the day. For further particulars consult the memoir published by Gauss in 1836, and translated in the second volume of Taylor's Scientific Memoirs*.

But thus far the inquiry had been comparatively restricted: the vastness of the problem required a larger field,—the whole of Europe, and still more the portion of it over which the magnetic observatories extended, being very small as compared with the entire globe. To study the problem adequately, it was necessary to determine accurately both the magnetic constants and their variations at several distant parts of the earth's surface: this was beyond the power of scientific associations and required the support of Governments. A first proposal of this nature had been made to the Emperor of Russia by Humboldt with the support of the Petersburgh Academy in 1819, with such success that magnetic establishments were formed in different parts of the Russian Empire, and even as far as China. A still wider range was however requisite; and the same celebrated individual, aided by the British Association for the Advancement of Science, and by the Royal Society of London, applied to the English Government as peculiarly interested in all that relates to navigation, and as having within its own dominions points of the globe which could furnish science with the desired information. The request was met with the greatest alacrity, promptness and liberality.

Two complete magnetic observatories were immediately established at Dublin and Greenwich, one under the direction of Dr. Lloyd, the other under that of Mr. Airy. For the distant stations four magnetical and meteorological observatories were erected at points suggested by Humboldt, Herschel, Airy and Sabine, as the best suited to disclose the laws of the phænomena. They were chosen so as to include great variety of magnetic intensity, and opposite positions in respect to the magnetic poles and to the magnetic and geographic equators.

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* The Scientific Memoirs referred to contain translations of several of the most important memoirs on the subject of terrestrial magnetism, with descriptions of the apparatuses and methods of Gauss and Weber.

One was established at Toronto in Canada, and another at Van Diemen's Island; these two points were chosen as being near to the two points of maximum magnetic force, and because, being almost antipodal to each other, they were eminently suited to show the differences which the opposition of the seasons in the two hemispheres might introduce in the variations.

A third observatory was placed at the Cape of Good Hope as the southern extremity of the African continent, and a point important for the large amount of secular change taking place in the magnetic elements.   Lastly, the fourth was established at St. Helena as a point situated within the tropics, and at a small distance from the magnetic and geographic equators, as well as from the line of minimum force.

The system of observations established at these four places was in the highest degree such as science might demand, and as should form a true monument to posterity. The instruments were all of the best dimensions and greatest precision, not only for the declination, but also for the inclination and intensity of the force. They included a large declinometer, a bifilar magnetometer for the horizontal force, a dipping-needle, a differential balance magnetometer for the vertical force, and a series of smaller supplementary instruments, with which the magnetic constants might be determined from time to time without touching the large magnets, which experience had shown ought never to be displaced.   All these instruments were furnished with mirrors or collimators and microscopes, and were observed from a distance with telescopes so as not to disturb their action.

The observatories were erected in open situations and without iron fastenings.   All were furnished with proper astronomical instruments for the exact determination of time, and with a very complete series of meteorological instruments to be observed at the same times as the magnetical ones.   Precise determinations were made from time to time of the different constants required for reducing the magnetic observations, such as the torsion of the thread, the coefficient of the variations of the force of the bars corresponding to variations in their temperature, the azimuths, meridian marks, &c.

Even this rough outline of the system must surprise the reader, and it may be safely said that there was never undertaken for the study of any physical problem a larger scheme or one less restricted by considerations of cost.   It was arranged that the action of the different observatories was to be simultaneous; their observations were made at first every two hours, and afterwards at every hour of exact Göttingen mean solar time, besides observing *at least* every five minutes whenever any extraordinary perturbation was perceived either in the needle or in the atmo-

sphere. That such a series of observations might be carried on with due precision, a military service was organized (under the general direction of Colonel Sabine), in which the observers succeeded each other day and night. They were taken from the corps of Artillery, selecting among the most intelligent those who showed most taste for the work. The staff of each observatory consisted of at least four non-commissioned officers and two soldiers, under the direction of a captain fully instructed in the duties to be carried on. The series was maintained for several years; in some cases more, in others less, but nowhere for less than five years.

If the quality of the instruments, the system of distribution of the observatories, and above all the moral character of the nation, constant and assiduous in carrying through whatever it undertakes, be considered, it will be owned that here was ground for expecting the manifestation of any discoverable laws, and that such data deserve full confidence as a basis of serious discussion. But undigested masses of figures are of small profit to science, and remain always a useless incumbrance in libraries and archives, if they are not *reduced, examined,* and *discussed*.

This second part of the work, perhaps not less laborious than the first, and requiring more ability, was also confided to the care of Colonel Sabine; and under his direction the numerous calculations of the reductions have been made, and published in nine large quarto volumes, at the expense of the British government, and which have been liberally distributed; the last was published a few months ago, and more volumes are in course of publication.

Besides the numerical calculations, Colonel Sabine has given, in the Introductions to these volumes, graphical representations of the variations at each place, and has compared the results from the different parts of the world, in order to deduce from them the most important conclusions. The whole has been executed with surprising sagacity, and at the same time with the greatest reserve as to theoretic hypotheses, a thing difficult in such matters, but strictly requisite where the fundamental data of a science have to be fixed. As this work is the most extensive of those hitherto published in tabular reductions, and the most important in respect of the points of the globe occupied, so it will be our principal point of departure in the deduction of laws*. We shall not however omit putting under

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* The work of reduction is of the highest importance, and it is necessary that it should be executed by those who have made or superintended the observations, to avoid the multifarious doubts which arise in its course. But in such reductions it is not sufficient to give annual and monthly means; it is also necessary to give graphical constructions, which are of

contribution the work done in other magnetic observatories, and especially in that established at Makerstoun in Scotland, at his own expense, by Sir Th. Makdougall Brisbane, and reduced by Mr. Allan Broun, a work valuable for having been performed for the express purpose of examining whether the heavenly bodies, and especially the moon, influenced the needle.

The Russian works and publications are also very important, because made in observatories formed on the same system as the English, and now published regularly from 1841, under the care of M. Kupffer, Director of the Central Physical Observatory of Russia. They form a collection of many volumes, and a special one, published in 1852, contains the results of the Magnetical and Meteorological Observatories of Petersburg, Catherinenburg, Barnaoul, Nertchinsk and Sitka, besides an immense number of data registered in upwards of twenty volumes, published under the title of *Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines, St. Pétersbourg,* which give all the observations made in the Russian empire and at Pekin.

Adding to this the many magnetic observatories established at various points in Italy, and (more especially) in Germany, in Holland, Sweden, the United States of America, and elsewhere; the many observations made and collected by the indefatigable and learned Quetelet, inserted in the Transactions of the Academy and Observatory of Brussels; and lastly, the long series made by Arago at Paris,—and it will perhaps be seen that there is no branch of physical science in which more laborious and copious researches have been made than in this.

A good part of the magnetic observatories which have been cited, and especially those of the British Colonies, are now out of activity; but the fruits collected remain, and it will be difficult in future to do more, or at least more useful work, than that which has been done. Rather a wise and intelligent œconomy requires that there should first be derived as much as possible from what has been obtained, in order to find out what direction should be given to new researches, when the materials afforded by the old shall be exhausted. Until such a period shall arrive, it is sufficient for the progress of science to have here and there a few centres of observation, and at this moment several such subsist. The most imposing of these is that of Greenwich. Its system of observation by photographic registration is the

immense assistance in the analysis of the phænomena. If all the magnetic publications had been furnished with reductions of this kind, we should have been able greatly to enrich the present memoir, but unfortunately they are rendered useless to us by the total or partial want of such a preliminary digest, which we have not ourselves the time or the means of supplying.

only one which can faithfully and fully illuminate the labyrinth of magnetic observation. I will briefly notice in what the apparatus, which has been working since 1847, consists, and will describe what was registered in the volume of the Meteorological Observations of Greenwich for 1847, and which I saw in 1849 through the kindness of Messrs. Airy and Glaisher.

The magnetic observatory is erected in a garden adjacent to the astronomical observatory; and the instruments, of which the march is photographically registered, are the declinometer, bifilar and balance magnetometers, and, for meteorology, the barometer, dry- and wet-bulb thermometers, &c. To speak here only of the magnetic instruments, the principle of registration is the following. Each magnetic bar has attached to it a small mirror, which receives through an aperture the rays of a gas-light passed through vapour of naphtha, to render its photographic action more efficacious. The mirror reflects these rays on a lens, which concentrates them in a bright point on a prepared photographic paper. This paper is rolled round a cylinder, which is moved by clock-work, and makes an entire turn in twelve hours, having its axis of rotation parallel to the line which the magnetic needle, reflected from the mirror, tends to trace as it vibrates. At the slightest oscillation of the needle, the reflected ray changes its place on the paper, and makes an impression on it; and this movement of the reflected image being compounded with the movement of rotation, there is produced on the paper a curve, of which the ordinates represent the amounts of the variations, and the abscissæ their times. If the needles are much disturbed, the paper is changed every twelve hours; if not, two curves are obtained on the same sheet. In order to have fixed points, and a base from which the times and ordinates may be counted, there is a second light, which sending its rays directly on the paper through a fixed hole, traces on it a straight line, to serve as the axis of the abscissæ; slight interruptions are made in the curve at intervals, by intercepting the light, so as to give points of departure for counting the times of the movements without error, and independently of the possible irregularities of the clock-movement.

The sheets on which these curves are traced are fixed by known photographical processes, and carefully preserved; and there are afterwards taken from them, by means of proper scales of reduction, the numerical data. Such a collection of observations cannot be otherwise than highly profitable. Science however expects its rigorous discussion with impatience. The apparent irregularities, which are numerous, especially in the more northern observatories, will disappear by the multiplication of curves; and although their first aspect may be discouraging, we shall see

that by taking a good clue as guide, it will be found here also that all is number and measure.

From the rapid sketch which I have traced of what has been already done for the study of the variations of terrestrial magnetism, it will be clear that observations are not wanting; and what is more, that they have been made systematically and with extraordinary perseverance, guided by experience, and with unexceptionable instruments; whence we may conclude, that if there were obtained at any place results opposed to the laws deduced from so imposing a mass of data, amounting to several millions, they ought to be attributed to faults either in the instruments, the observations, or the methods of reduction; in short, to some local or accidental cause; but we shall see that the results are more accordant than would be believed, and it is of these that we now proceed to speak.

Part II.—*Principal results obtained from the study of the Magnetic Variations.*

Magnetic variations are of three kinds; the first ordinary and periodical; the second extraordinary, and apparently irregular; and the third requiring for their completion a great and unknown series of years, and therefore termed *secular*. All the magnetic elements, *i.e.* the Declination, the Intensity of the Force, and the Inclination, are subject to these variations. We will begin with what belongs to the declination and its movements, which fall under some principal laws, which we will proceed to expose.

§ 1. *On the Diurnal and Annual Variations of the Declination.*

First Law.—"The diurnal variations of the magnetic needle follow local time."

Declaration.—The first discoverers of the diurnal variations of the magnetic declination suspected that the needle followed the course of the sun, and therefore the true (or apparent) time of the place of observation; but when it was afterwards found, by comparative observations, that there were cotemporaneous variations at many different places, it was suspected that there might be simultaneity of perturbation throughout the globe. When however places of observation sufficiently distant were multiplied, it was found that the ordinary, or diurnal variations, followed in their march the hours of local time, and that even the extraordinary variations, as we shall see in the appropriate place, were not completely excluded from the operation of this law. To avoid speaking equivocally, however, the term "distance" must be understood in relation to the subject of which we are treating. The extent even of the whole of Europe, and still more distances

of six or seven hundred miles, are very small compared to the entire circumference of the globe. In the same manner that many meteorological vicissitudes may be simultaneous for such extents, so may also the magnetic perturbations which might be produced by them; but as it can rarely happen that meteorological causes occupy the whole surface of the earth, so simultaneous perturbations produced by them and extending over the whole globe would be equally rare.

In fact, if we inspect the magnetic curves, traced in Göttingen time, for Göttingen and Prague in Europe, and for places situated in Canada and the United States of America, we shall find that the places in each continent commonly agree very well with each other; but that agreement between the continents is seldom found, although their distance apart is not great compared to the whole globe.

It is necessary however to discriminate accurately between two kinds of periodical variations; those which strictly follow local time, and those which in their periods occur at the same moment of absolute time at different stations. We shall speak of the latter subsequently; but in respect to the former, let it be regarded as assured that local time is to be alone considered; and that if Göttingen time was at first adopted for all the observatories in common, it was for the sake of making out the law of the extraordinary movements and facilitating their calculation, rather than for the purpose of recognizing the law of the diurnal variations of which we are now speaking.

It would indeed have been desirable to have adopted *true* or *apparent* local solar time, instead of mean time, in the observations, or at least in the reductions. The use of mean Göttingen time, besides the inconvenience of requiring the equation of time to be applied, has also another, which is, that it does not often happen that at two distant observatories the observations fall at even hours of local time. This is one of the points (and we shall see others presently) in which the discussion of past observations throws light on the system to be adopted in future. It is to be hoped that future observations will be made at even hours of apparent local time, and that those which have been made will be reduced to such hours.

We have however found, and shall demonstrate in the sequel, that the phase of the diurnal oscillations depends more on the position of the sun relatively to the magnetic meridian of a given place (*i. e.* relatively to the azimuth of the plane of the magnetic meridian) than on the relation to the geographic meridian.

Second Law.—" The pole of the needle which is least distant from the sun makes a double diurnal excursion, in the following manner:—It is at its maximum of western excursion four or five

hours before the sun passes the meridian of the place; it then turns eastward with increasing celerity, of which the maximum occurs near the passage of the sun through the magnetic meridian, and it reaches its limit of eastern excursion one or two hours after the said passage. As the sun declines, the needle returns back; and as the sun passes the inferior meridian, there is repeated in the night the same variation as that which took place during the day, but restricted within narrower limits. The limiting hours of these changes vary with the seasons, and are generally earlier in summer and later in winter; and the magnitudes of the excursions are in the proportion of the diurnal to the nocturnal arc."

Declaration.—The wood-cut will make this law better understood. Let EW be the equator or the parallel described by the
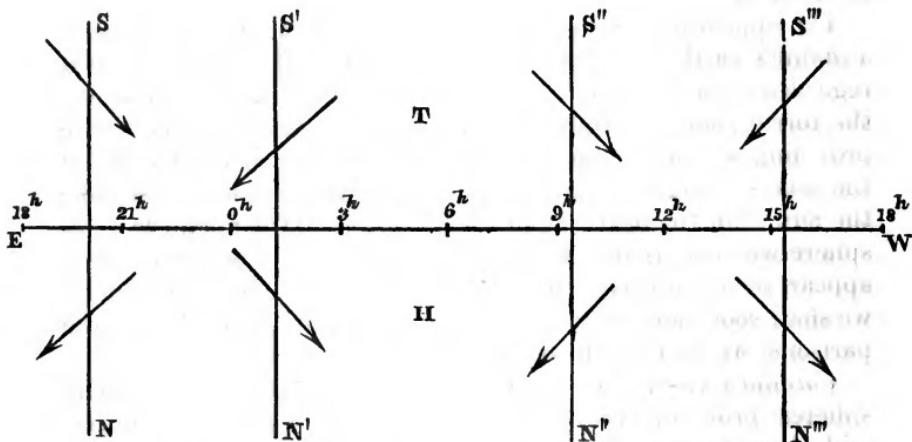
sun, and T, H, two places situated in opposite hemispheres as respects the sun; between the hours of 19^h and 21^h (7 and 9 A.M.) the two needles will be in the positions shown on the line SN; from 1 to 2 P.M., in the position shown on the line S'N'; between 9 and 10 P.M., as shown on the line S''N''; and, finally, between 14^h and 15^h (2 and 3 A.M.), as on the line S'''N'''.

The case here represented is exactly that of Toronto (T), and Hobarton (H). Colonel Sabine describes the diurnal oscillation at Toronto as follows:—"The mean diurnal variation of the declination at Toronto, as derived from the two-hourly observations in 1841 and 1842, consists in an easterly movement of the north end of the magnet from 2 to 10 hours inclusive; a small return movement towards the west then takes place till 14 hours, when the easterly progression is resumed, and continues until 20 hours, at which time the north end of the magnet reaches its eastern limit. From 20 hours the movement is continuous towards

the west until 2 hours, which is the period of the extreme western limit." (Toronto Observations, vol. i. p. 14.) And at Hobarton as follows :—" The north end of the magnet has two eastern and two western elongations or turning-points, at both periods of the year; from October to February the principal eastern elongation is at 2^h, and the minor one at 15^h; from April to August the hours of these turning-points become respectively 3^h and 16^h; from October to February the principal western elongation is between 20^h and 21^h, and the minor one at 11^h; whilst from April to August the corresponding phænomena occur at 22^h and 11^h." Then, comparing the figures which represent these movements with those of Toronto, he concludes that they are identical, only having opposite signs, *except that the turning-points or periods are earlier at Toronto than at Hobarton.*

The opposition of these movements is shown in our figure in a manner easily to be remembered. The two stations may be regarded as [within limits, ED.] the type of all that happens out of the torrid zone. Within or near the tropics the law holds good, providing we have regard to the hemisphere in which the sun is, the places being considered as in the southern hemisphere when the sun is in the northern hemisphere, and in the northern hemisphere when he is in the southern. If there should sometimes appear to be an exception, it would be only an apparent one, as we shall soon demonstrate. In the meantime, to facilitate comparisons, we may establish the following :—

Corollary 1.—All the variations are the same in both hemispheres, providing we change the name of the pole influenced; and if we take as the type the north pole and northern hemisphere, we shall have identical variations for the south pole in the southern hemisphere; and the variations of the north pole in the southern hemisphere will be opposite to those in the northern hemisphere.

Remark.—Perhaps, to avoid any misunderstanding, and the confusion of poles with hemispheres, and for greater convenience in the indication of the antagonistic forces of poles in which the pole *called north* is the *true south* pole of the needle, it might be better to retain the name of *marked* pole, formerly used by some, and especially by English writers, to designate the fundamental pole, to which all is referred, and which *in our* part of the world looks to the *north.*

Corollary II.—As the points of inflexion of the diurnal curve depend on the sun's passage of the magnetic meridian, it follows that if two places in the northern hemisphere have opposite magnetic declination, *i. e.* the one east and the other west declination, the second will be in its phases later [*Qu.* earlier, ED.]

than the other. If the two places which we are considering are in opposite hemispheres, this new opposition will have to be taken into account, that is to say, we shall have to make the product of the algebraical signs relatively to their positions and to their names. This rule will be useful to us presently. It is a consequence of the complete antagonism which exists in the two hemispheres relatively to magnetic phænomena.

Corollary III.—A consequence of the dependence on the magnetic meridian is the advancement or retardation of the phases with the seasons, as in the course of the year the sun arrives at the same azimuth from the geographical meridian by describing a different horary angle, greater in winter and less in summer; but the discussion of this point will be more suitably placed in the consideration of the elementary movements of the needle.

We will not however omit to notice here one important point. The needle, in its nocturnal oscillation, and especially in winter, makes an excursion which sometimes exceeds the diurnal one. This has sometimes caused it to be believed, that the maximum of deviation, especially of western deviation, was subject to great displacement. But the case is otherwise. The proper maxima of the semi-diurnal excursions always remain at nearly the same hours; but if it should happen that the nocturnal should exceed the diurnal, we are not therefore to say, without qualification, that the maximum occurs in the evening; the times and the periods are to be distinguished, and all will be clear; for if the *absolute* maximum may happen at night, the relative maxima however (eliminating the perturbations) follow constantly the period above enounced; and it is these relative maxima and minima which constitute the characteristic properties of the variations of the magnetic as distinguished from the meteorological period.

Remark.—The two laws hitherto enounced are themselves no other than corollaries of another more general law, which we will now proceed to expose; but I have thought it well to premise them, and to enunciate them separately, in order to proceed afterwards with greater clearness.

Third Law.—" The diurnal excursion of the needle is the sum of two distinct excursions, of which the first depends solely on the horary angle, and the second depends besides on the sun's declination. These two fluctuations being variously superimposed upon each other, produce by their interferences all the phænomena of the ordinary diurnal and annual variations."

Declaration.—Nothing is in appearance more bizarre than the curve traced by the magnetic needle in a single day; but as there is no real irregularity in nature, it is obvious to presume

that the appearance of irregularity only arises from our being ignorant of the fixed periods, as well as of the accidental causes which influence the needle. Desiring to treat this subject with systematic order, it is necessary to restrict ourselves to the regular variations only. In order to give an idea of the annual variations of the declination without too much multiplying words, we will refer to Sabine's work (Toronto, vol. ii. p. 20) for the curves traced by him, representing the position of the needle in the two six-monthly periods when the sun is on either side of the equator, at the four observatories of Toronto, St. Helena, Cape of Good Hope, and Hobarton.

In these figures the red line indicates the excursions of the needle in the months when the sun is in the northern signs, or in the tropic of Cancer; and the blue line the same in the months when the sun is in the southern signs, or in the tropic of Capricorn. These curves include only the hours of the day, as being the most marked. The north pole of the needle deviates to the east when the curve is above the axis of the abscissæ, and to the west when it is below it. From a simple inspection of these curves, we may draw the following conclusions:—

At Toronto, the needle at 8 in the morning is throughout the year to the east of its mean position; and in the afternoon, towards 2 P.M., it is always to the west; 2nd, the excursion is greater in summer than in winter, and the annual difference in this respect is represented by the distance between the two curves; 3rd, in the intermediate months the needle is between the two limiting curves.

For Hobarton we get the same laws, but with contrary denominations, as we have already said (under Law II. Corollary II.).

For St. Helena there is the notable circumstance, that the curves are seen to bend alternately south and north of the equator, moving with the sun; yet it is not to be overlooked, that the curve of the months of the June solstice wants the second inflexion, which it would require in order to be symmetrical with the curve of the opposite six months.

At the Cape of Good Hope the phases are transitional between those of St. Helena and Hobarton. These curves are the graphical result of the observations, and we have now only to see whether it is possible that they may have originated from more simple periods, which, being separated from each other, may throw light on the physical cause of the phænomenon.

These curves are traced by taking the mean of the six months, and hence they appear more regular than if taken from the different months singly; for if we examine each of the constituent monthly curves separately, we shall find some peculiarities and notable differences, which tend further to confirm the belief that

all these curves conceal simple periods, which being superimposed give complicated results.

That such periods being superimposed upon each other may produce curves of irregular appearance, will not be doubted by any one who may have once seen the multifarious curves, obtained by the superimposition of one or two waves, in the little machine invented by Wheatstone for representing the interferences of luminous undulations; and it is just the application of these principles to the theory of terrestrial magnetism which reduces these facts, in themselves highly intricate, to a surprising degree of simplicity. In order to render more intelligible what we are about to say, it will not be without its use if we conceive a wave of which the elementary curve is the ordinary one of simple sines, having for its equation

$$y = k \sin (x + a),$$

and a second wave of double period, and of the equation

$$y = k \sin (2x + a).$$

If we superimpose these two forms, we shall have a figure distinct from either. We here suppose the two components to have *equal excursions*; but by giving different values to the constants which enter into the curve, we may get the share belonging to the minor diurnal inflexions to be almost sensibly rectilinear; and *vice versâ* we may have more exaggerated inflexions.

In the equations of these curves we will distinguish the constants by special names for the sake of brevity and clearness, calling k the modulus, the arc x the argument, and a the parameter.

This being premised, we come to the demonstration of the law which has been enounced, which will be no other than a corollary of the observed facts.

And first, from an extended and comparative analysis of all the magnetic observations, the sun is seen to be the principal cause, not only of the diurnal, but also of the annual variations; and we have only to form to ourselves a clear idea of the manner in which it operates. Colonel Sabine, in vol. ii. of the Toronto Observations, p. 20, briefly sums up the fundamental points, comparing the curves which we have cited, and calls attention to two things,—1st, the opposition of the movements of the needle in the two observatories situated beyond the tropics in the two opposite hemispheres (*i. e.* at Toronto and at Hobarton); and 2nd, the opposite direction induced by the sun's passage of the equator in the declination of the needle at St. Helena and at the Cape of Good Hope, which phases place beyond doubt the influence of the sun's declination. He does not however proceed further with the analysis. Now it seemed to me that this germ might be considerably more developed, and might become fertile in very important consequences. It seemed to me strange

that the sun should act thus oppositely by his change of declination in these two places and not in the others, limiting itself in these last to only diminishing the fluctuations. It was added, that the changes at St. Helena and at the Cape not having reference to the sun's zenith-distances, his influence ought to be due to an astronomical rather than to a geographical and local cause. It may however naturally be expected that such a period is marked by the many convolutions and superimpositions of different causes acting on the needle; to extricate it was not easy, and would have been actually impossible without the previous labours of Colonel Sabine, which I have happily found sufficient for the purpose.

<div style="text-align:center">[To be continued.]</div>

L. *On the Electricity of Flame.* By M. Matteucci[*].

My dear Grove,

I HAVE gladly taken advantage of a few days' leisure in the country to comply with your request, and at the same time satisfy my own curiosity, by making some experiments on the electricity of flame, and I now send you the little which I have gathered from my own observations on the subject. I began by studying the conductibility of flame, and employed for this purpose a galvanometer constructed by Ruhmkorff, similar to that of M. du Bois Reymond, the fine copper wire of which makes 24,000 evolutions round a good astatic system. The flame used in most of my experiments was that of a common spirit-lamp, or of a double current lamp belonging to the laboratory. The circuit, which contained two Daniell's couples, was formed by two platinum wires, two-thirds of a millimetre in diameter, placed horizontally, and leaving between them a fixed interval of 8 millims. All was perfectly insulated, and the flame completed the circuit. On seeking to ascertain the conducting power of the different parts of the flame, it is soon found that this conductibility increases rapidly in going from the base or root of the flame towards the summit. To give an idea of this difference, I mention here that I had from 4° to 5° of deflection at the base, 8° to 10° at the middle, and from 30° to 40° at the upper part of the flame. By placing the platinum wires in the flame of a double current lamp, much more striking effects are obtained, which are evidently in proportion with the temperature of the gas composing the flame. If we hold the platinum wires at a less interval apart, that is, from 2 to 3 millims., over the flame of the double-current lamp, which can be diminished at pleasure, we can ascertain the precise moment at which the conductibility of the flame begins; I found in my experiments that this moment

was that in which the two wires are of a clear red, without having been visibly reached by the flame. This is, as far as I remember, the result obtained by M. E. Becquerel, and accords with what we know to be the case with all bodies excepting metals, that is, that their conducting power increases by heat. Now is this an effect similar to that undergone by liquids, in which conductibility and electrolyzation go on together, or is it rather a discharge like that of the voltaic arc, favoured chiefly by the high temperature of the electrodes which are thus disposed to a disaggregation? I have often seen a great deflection of the needle persist for a certain time, when the wires heated to a white heat are moved from the upper to the middle portion of the flame, but this is a subject which can only be discussed by the help of other experiments, which I hope that you, who have begun this subject so successfully, will find time to undertake.

When, instead of an alcohol flame, that of oil or stearic acid is employed, we find at first that the passage of the current is nearly null; but by taking means to prevent the formation of a stratum of black matter on the electrodes, we soon find that the conducting power of these flames is greater than that of alcohol. In order to effect this, I heat the platinum wires in a spirit-flame, which I replace rapidly by that of the substances just named, and I then find that the conductibility of these flames is much greater than that of alcohol. Proceeding in the same manner, I ascertained that the conducting power of the flames of camphor or of gutta percha was much less than that of alcohol.

If between the electrodes a thin plate of platinum is interposed, which may be said to cut the flame in half, the conductibility remains unaltered; and if this plate has been heated previously, we find, on the contrary, that the conducting power is increased. By making use of salt of strontian, or boric acid, or chloride of copper dissolved in small quantities in alcohol, I have been enabled to work with flames which, as is known, present different colours due to volatilized matter: the conductibility of these flames is greatly diminished, and this is particularly the case with the flame rendered green by chloride of copper.

The conducting power is visibly increased by a jet of vapour of iodine or of mercury sent into the midst of the flame, and the power is lessened by a jet of steam from water, or by holding burning sulphur in contact with the flame. It would be very curious to study the conducting power of steam at different atmospheres and densities, and above all, to ascertain whether this conductibility is accompanied by electro-chemical decomposition.

I now proceed to the development of electricity by flame. I had no difficulty in verifying the results which you have obtained, and which I will express as follows:—We find in the flame

of alcohol or of hydrogen gas, and probably in all flames, an electric current, the direction of which current in the flame itself is from the portion called by chemists *reductive* to that called *oxidizing*; in other words, the current of the flame is directed from the metallic plate which is in contact with hydrogen or vapour of alcohol, to that which is in contact with oxygen or atmospheric air. You have clearly proved that this current does not depend on the difference of temperature between the two platinum wires placed in the flame, for it is obtained when the wire surrounded by oxygen is held above the flame so as to be less heated than the other. It may be added, that the current obtained by touching directly the greater and the less heated platinum, would only occasion a deflection of a few degrees, instead of which, in operating with the flame, we have a deflection of 40 or 50 degrees in an opposite direction. The currents obtained by employing plates of platinum are much stronger than when wires are used, and the best arrangement is that of holding one of the platinum plates so as partly to envelope the flame, while the other is placed in its centre. I have obtained similar results, though the currents have been rather weaker, with copper or iron wires, and from the state of these wires we can judge more distinctly of the portion of the flame which contains hydrogen, and of the external part where oxygen predominates.

As to the explanation of the production of electricity in flame, I do not for a moment hesitate to consider the case as identical with that by which I think I have explained M. Pouillet's old experiment and secondary polarities, which formed your starting-point in your beautiful discovery of the gas battery; it is that of an electrical current developed between two metallic plates, and principally between two plates of platinum when plunged in water, being or having been in separate contact, the one with hydrogen, the other with oxygen gas. You are aware that, in the case of flame, I have supposed that these two gases were separated by a stratum of steam. It is known that the electrostatic signs of flame accord perfectly with those obtained with the galvanometer, as well also as with those from the gas battery.

Before coming to the experiment, which seems to me to leave no doubt as to this interpretation of the electrical phænomena of flame, allow me to describe some experiments on electrical currents, obtained with platinum wires attached to the extremities of the wires of the galvanometer, and immersed in distilled water. I take a platinum wire 3 or 4 metres in length; after having left this wire for some hours in a mixture of nitric and hydrochloric acids, I wash it repeatedly in hot water. I then cut this wire in two equal parts, and join each half to one of the extremities of the galvanometer which I have already described.

If I plunge the two ends of platinum wire into distilled water at the same time, the needle seldom remains at zero, but generally after a deflection of a few degrees returns to zero, the circuit having been left closed. If I then withdraw the two wires simultaneously from the water, and after some minutes plunge them again into the water, there is no deflection of the needle. The same result is obtained if the two wires are wiped dry with linen or paper before replacing them in the water. I have succeeded in obtaining very strong and constant currents in the following way: if one of the wires is moved rapidly in the liquid, if this wire alone is withdrawn from the water and after some time replaced, if the wire is wiped and rubbed with linen or paper and then put into the liquid, if, finally, this wire has been heated over charcoal,—in all these cases there is a strong current on closing the circuit, and the direction of this current shows that the wire which has undergone these changes becomes as the zinc of the voltaic battery. It is easy to show, as may be done with platinum plates which give secondary polarities, that these currents exist only in presence of the liquid interposed between the two platinum wires. In order to prove this, we have only to withdraw the two wires from the water, and then to heat or rub one wire more than the other; if then this wire is plunged into mercury, no current is obtained, whereas by employing water a current is obtained. In order to explain these results, we must admit with M. De la Rive, that platinum decomposes, or tends to decompose water at the ordinary temperature, and we may suppose that this tendency is increased by the circumstances which I have described.

I will now give, in the last place, the experiment which serves to explain the production of electricity by flame. I place two platinum wires so that one is in contact with the centre of a flame of a spirit-lamp, and the other with the outer margin or apex of this flame. I make the experiment as before described, and the needle, as we already know, deflects so as to indicate a current the direction of which is from the base towards the apex of the flame. After a certain time I take away the flame, I leave the wires to cool, and close the circuit with water. I have then a strong current in the same direction as when I employed the flame. It is hardly necessary to tell you that I assured myself of the truth of this result by alternating the position of the two platinum wires in regard to the flame, as well as by holding the two wires at the same level in the flame, in which case no current is obtained either with flame or with water. I have also ascertained in this experiment, that when the two wires, after being kept in the flame, are put into mercury, no current is obtained, whereas when they are plunged into water there is a strong current.

I do not therefore hesitate to conclude, that the electric current

which you obtained from the flame of alcohol or of hydrogen is of the same nature as that which is obtained if the two platinum wires are placed in water after having been in contact, or while in contact, the one with hydrogen, the other with oxygen gas : the current in the former case is feebler, owing to the bad conducting power of flame. We should thus be led to suppose,— 1st, that the action between the platinum and the gases takes place even at a very high temperature, which supposition is in accordance with experiment; 2nd, that vapour, like water in a liquid state, is decomposed by the electrical current, which remains to be proved.

I must not close this letter without observing that I am not, nor has there been time for me as yet to be, acquainted with the works of the German savants Buff and Hankel, of which you speak. Should it appear to you that these few observations offer any interest, I should feel greatly obliged by your communicating them, as a proof of my gratitude, to the Editors of the Philosophical Magazine.

Ever yours,

Pise, September 26, 1854.   C. MATTEUCCI.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In communicating the above interesting experiments, I am tempted to add a few words of comment; for though I do not differ much from the interpretation of the flame-current given by my friend M. Matteucci, it interests me in a different point of view. This I can best explain by stating a conviction I very early acquired from the chemical theory of Voltaism, and which has led to many of the experimental results that I have from time to time communicated to the Philosophical Magazine. It is, that every chemical synthetic action may, by a proper disposition of the constituents, be made to produce a voltaic current.

This proposition may probably appear to many a direct and necessary consequence of the chemical theory, but I do not recollect to have seen it broadly stated. Thus the dissolution of gold by aqua regia I showed might produce a voltaic current. The gas battery was, in my mind, a similar deduction, and was arrived at by considering the chemical action in the Döbereiner lamp, and not by the experiments of M. Pouillet and M. Matteucci, with which I was unacquainted at the time of my first experiments, though doubtless those would have been an equally ready step to the result. The production of a voltaic current from phosphorus and iodine, solid non-conductors, the nitric acid battery, &c., were derivatives of the same thought.

It has often occurred to me, that if, instead of using zinc and

acids, which are manufactured, and comparatively expensive materials, for the production of electricity, we could realize the electricity developed by the combustion in atmospheric air, of common coal, wood, fat, or other raw material, we should have at once a fair prospect of the commercial application of electricity; but in the experiments I have mentioned, and others of the same class, an electrolyte is a *sine qua non.*   Hence I was led to experiment on flame, which appears to me to differ from the above in the absence of an electrolyte; but M. Matteucci seems, if I rightly read his letter, to regard the steam in the flame as having an electrolytic action, being, in fact, the substitute for the water of the gas battery.   Here I to some extent diverge from his views.   Water in the liquid state cannot exist in the blowpipe flame, it must be in the state of vapour or gas, and the transfer would not be electrolytic, the which, to my mind at least, involves the idea of a liquid.   Vapour is, as I have shown, decomposed by the electric spark, but not, as far as I am aware, by the mere transfer of electricity.

It would be very hasty to say a thing cannot be because it has not been, but I cannot at present assume theoretically the electrolysis of steam, and I believe the general opinion would be against it; it may be polarized, but not, I think, electrolysed.

It does not, however, seem to me that this hypothesis is needed; there is throughout the course of flame a chemical action going on, each molecule of carbon or hydrogen is combining with a neighbouring molecule of oxygen, and there is nothing which theoretically opposes the supposition that such a chain of chemical action should conduct electricity, even though the gases in a chemically dormant state should not do so.   As there is more oxygen at one extremity, more hydrocarbon at the other, there must be, irrespective of the molecular combinations, which do not contribute to the voltaic current, a certain number of particles as to which the chemical action has a definite direction; this, to my mind, should produce an electrical current, which should, as it does in fact within certain limits, increase in intensity with the length of the interposed flame.   Flame to me presents a different medium, electrically viewed, from water or from gas, whether simple, compound or mixed, and this gives it a high interest.

I will take this opportunity of stating, that with the flame battery which I used for an evening meeting at the Royal Institution, I readily decomposed iodide of potassium, a result I had not obtained when I made my former communication to the Philosophical Magazine.

<div style="text-align:center">I have the honour to remain, Gentlemen,</div>

<div style="text-align:right">Your obedient Servant,</div>

Etrétat, France, Oct. 6, 1854.   W. R. Grove.

LI. *Intelligence and Miscellaneous Articles.*

ON SOME NEW DERIVATIVES OF CHLOROFORM.

BY PROF. WILLIAMSON.

ACCORDING to the results of recent researches in the constitution of salts and the methods thence introduced of explaining chemical reactions, it is equally correct to represent such a reaction as that of hydrochloric acid on hydrate of potash, as consisting in an exchange of hydrogen of the one for potassium of the other, or of chlorine in one for peroxide of hydrogen in the other. In Mr. Kay's researches as described in the following brief outline, this notion has obtained very striking illustration; for he has obtained a peculiar body in which the chlorine of chloroform is replaced by peroxide of æthyle by the action of chloroform on three atoms of æthylate of sodium, which product may be equally well conceived to be a body in which the hydrogen of three atoms of alcohol is replaced by the tribasic radical of chloroform.

According to the older theories of the capacity of saturation of salts, this compound would contain a tribasic modification of formic acid, for it has the same relation to formic æther as a so-called tribasic phosphate has to a monobasic one.

To one equivalent of chloroform were added, by degrees, three equivalents of dry and powdered æthylate of soda, a violent action taking place with the evolution of much heat; the liquid was entirely distilled from the residue (chloride of sodium) by means of an oil-bath, and then subjected to a series of fractional distillations, which yielded a small distillate between 50° and 60° C., smelling strongly of vinous æther, a large distillate (about three-fourths of the whole) between 77° and 78° C., which was chiefly alcohol, and another small distillate (about one-sixth) between 145° and 145°·3 C.

The distillates obtained by the above process, except that of alcohol, being small, the following modification was adopted.

Sodium was dissolved in absolute alcohol until the action became feeble, chloroform was then added, care being taken to keep the liquid alkaline; more sodium was then added, and the process repeated several times, until the chloride of sodium precipitated became very bulky. The liquid was then distilled off and chloroform added to the residue, and also distilled off. To this first distillate sodium was again added, and treated with the last distillate instead of pure chloroform, the same precautions being used as before. This method gave similar distillates, and in about the same proportion as that first used; the highest distillate however boiled constantly at 146° instead of at 145°·3 C.

This compound which boils at 145° to 146° C. is a colourless limpid liquid, only slightly soluble in water, having a strongly aromatic odour, readily inflammable and burning without much smoke; its specific gravity is ·8964; it remained liquid at 0° F.

Several analyses made of this body agree in giving to it the formula $C^7 H^{16} O^3$, which would also be the empirical formula of a tribasic formic-æther; the density of its vapour also corresponds very closely with the same formula.

Pentachloride of phosphorus added to a portion of the compound produced a heavy liquid having the odour of chloroform.

A small quantity of the body was dissolved in alcohol, distilled upwards for two or three hours with solid hydrate of potash and then distilled off; the residue was next dissolved in water and made exactly neutral by hydrochloric acid, filtered to remove the turbidity, and then a few drops of chloride of mercury added; after a little time and by the application of heat, a very slight precipitate of subchloride of mercury was formed; also the colour of sesquichloride of iron was a little darkened by another portion of the solution, thus showing that the action of potash on the compound had produced formic acid, but in very small quantity.

An equivalent of dry hydrochloric acid was passed into a portion of the compound; the gas was wholly absorbed, a considerable amount of heat being evolved and the liquid assuming a brownish colour; the liquid after the absorption of the gas still remained perfectly neutral. It was next distilled with the thermometer: it began to boil at 20° C. and rose gradually to 100°; it was collected in three portions, the first (about one-sixth of the whole) passing over between 20° and 50°, the second (about one-third) between 50° and 68°, the third (one-half) between 68° and 100°. I was unable to carry these distillations further in consequence of the small quantity of the liquid available.

Two equivalents of dry hydrochloric acid were passed into a larger quantity of the compound; towards the close the gas was absorbed less freely, a portion passing through; after this treatment, the liquid fumed and was highly acid; it was distilled upwards for some time, by which a portion of free hydrochloric acid was expelled, and then distilled fractionally; about one-third came over between 56° and 60° C., one-fourth between 60° and 70°, one-sixth between 70° and 80°, and the remainder (about one-fourth) between 80° and 88°. To the lowest distillate about an equal bulk of water was added; the substance floated on the surface and seemed to be little, if at all dissolved by the water; a sufficient quantity of carbonate of soda was next added to neutralize the free acid, and the liquid pipetted from the water, it was then distilled upwards for some time with dry chloride of calcium, and afterwards distilled off; this distillate was found to boil constantly at 55°·5 C. An analysis made of this body agrees closely with the formula $C^6 H^{14} O^5$.

The distillate which came over between 60° and 70° after being treated in the same way as the lower distillate, also yielded a liquid which boiled at 56° C.

As both methods hitherto used for the purpose of obtaining the body $C^7 H^{16} O^3$ afforded only small quantities, the treatment of chloroform with an alcoholic solution of potash was tried; for this purpose 12 oz. of solid hydrate of potash and 20 oz. of quick lime were added to about three pints of absolute alcohol, and the alcohol distilled upwards for six or seven hours; 6 oz. of chloroform were then added gradually, the upward distillation being continued about two hours longer; the liquid was next distilled off to dryness by means of an oil-bath, and submitted to fractional distillation; by this method a much larger quantity of the compound was obtained than

by the former processes; it was found to boil constantly at 146° C., and its analysis agreed almost exactly with the formula. In this process the lowest distillate had the same smell of vinous æther which was before observed in the other methods.

An attempt was made to produce the intermediate compounds C H Cl², AeO, and C H Cl, 2AeO, by adding dry and powdered æthylate of soda very gradually to a large excess of chloroform; but the liquid after being separated from the precipitate, was found, on distilling fractionally, to resolve itself into chloroform, alcohol, and the body ($C^7 H^{16} O^3$) already obtained, the presence of no other substance being observable.

With a view of obtaining a compound analogous to the body $C^7 H^{16} O^3$, in which amyle should be introduced instead of æthyle, dry amylate of soda was prepared, to three equivalents of which one equivalent of chloroform was added, the liquid separated from the precipitate and then distilled fractionally; a large proportion of fusel-oil was obtained, together with a small proportion of a body which boiled at a high temperature,—from 260° to 290° C., but chiefly from 260° to 270°; the purification of this substance was not carried further, as at each distillation a considerable portion was decomposed even in an atmosphere of hydrogen, the small quantity of the liquid available precluding any more attempts at distillation.—*From the Proceedings of the Royal Society*, June 15, 1854.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1854.

Chiswick.—September 1. Very fine. 2. Slight fog: very fine. 3. Foggy: very fine. 4. Foggy: slight haze: excessively dry air. 5. Very fine: hazy: fine. 6—10. Very fine. 11. Dense fog: clear: quite cloudless: very fine. 12. Dense fog: very fine. 13. Cloudy: rain. 14. Cloudy: slight rain: overcast. 15. Very fine. 16. Overcast. 17. Very fine. 18. Fine: cloudy: rain. 19. Overcast and windy: slight rain. 20. Cloudy: rain. 21. Clear: quite cloudless: fine: lightning in the evening. 22. Very clear: fine. 23. Densely overcast. 24, 25. Very fine. 26—29. Foggy, with very heavy dews in the mornings: very fine throughout the days: clear and cold at nights. 30. Dense fog throughout.

Mean temperature of the month .... 56°·93
Mean temperature of Sept. 1853 .... 55 ·45
Mean temperature of Sept. for the last twenty-eight years... 56 ·98
Average amount of rain in Sept. .... 2·57 inches.

Boston.—Sept. 1—4. Fine. 5. Cloudy. 6. Fine. 7—9. Cloudy. 10—12. Fine. 13. Cloudy. 14. Rain A.M. 15—17. Cloudy. 18. Fine. 19. Cloudy: rain A.M. 20. Rain A.M. 21, 22. Fine. 23. Cloudy: rain A.M. 24. Cloudy: stormy A.M. and P.M. 25—29. Fine. 30. Foggy.

Sandwick Manse, Orkney.—Sept. 1. Cloudy A.M.: clear P.M. 2. Clear A.M.: cloudy P.M. 3. Clear, fine A.M.: clear P.M. 4. Rain A.M.: clear, fine P.M. 5. Cloudy, fine A.M.: cloudy P.M. 6. Drizzle A.M.: rain P.M. 7. Drizzle A.M.: cloudy P.M. 8. Cloudy A.M.: clear, fine, aurora P.M. 9. Clear, fine A.M. and P.M. 10. Bright, fine A.M.: clear, fine P.M. 11. Rain A.M. and P.M. 12. Clear, fine A.M.: cloudy P.M. 13. Clear A.M.: rain P.M. 14. Clear A.M.: showers P.M. 15. Showers A.M. and P.M. 16. Showers A.M.: cloudy P.M. 17. Bright A.M.: showers P.M. 18. Showers A.M. and P.M. 19. Bright A.M.: clear P.M. 20. Showers A.M.: cloudy P.M. 21. Bright A.M.: clear P.M. 22. Bright A.M.: cloudy P.M. 23, 24. Rain A.M.: clear P.M. 25. Showers A.M.: cloudy P.M. 26. Showers A.M.: clear P.M. 27. Clear A.M.: cloudy P.M. 28. Clear, fine A.M.: clear P.M. 29. Cloudy A.M. and P.M. 30. Hazy A.M.: clear P.M.

Mean temperature of Sept. for twenty-seven previous years . 52°·32
Mean temperature of this month .... 55 ·07
Mean temperature of Sept. 1853 .... 53 ·28
Average quantity of rain in Sept. for fourteen previous years   2·81 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1854. Sept.	Barometer.					Thermometer.					Wind.			Rain.		
	Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Orkney, Sandwick.	Chiswick.	Boston.	Orkney, Sandwick.
	Max.	Min.		9¼ a.m.	8½ p.m.	Max.	Min.		9¼ a.m.	8½ p.m.						
1.	30·314	30·274	29·87	30·19	30·16	72	39	57	57	52	e.	calm	sse.	......	......	......
2.	30·353	30·339	29·87	30·13	30·13	74	37	59	58½	58	e.	s.	wsw.	......	......	......
3.	30·333	30·282	29·89	30·16	30·20	78	37	63	62½	56½	e.	s.	ssw.	......	......	......
4.	30·351	30·302	29·86	30·20	30·41	80	35	57	52	51½	e.	ssw.	wnw.	......	......	·06
5.	30·421	30·410	29·95	30·42	30·34	68	36	64	54½	52½	e.	e.	w.	......	......	......
6. ○	30·378	30·236	29·90	30·13	30·22	73	38	55	56	58	ne.	calm	ne.	......	......	......
7.	30·237	30·206	29·77	30·31	30·34	·76	46	64	52	51	e.	e.	ese.	......	......	·29
8.	30·221	30·176	29·77	30·30	30·23	67	43	58	55	52	ne.	nne.	ese.	......	......	......
9.	30·174	30·139	29·75	30·18	30·10	68	32	58	58	51½	ne.	n.	sse.	......	......	......
10.	30·191	30·101	29·74	30·02	29·93	68	33	61	57	57	ne.	calm.	s.	......	......	......
11.	30·174	30·082	29·74	29·81	28·84	73	31	52	61	58	se.	w.	sw.	·03	......	·28
12.	30·016	29·901	29·58	29·75	29·52	81	58	60	62½	56½	se.	s.	s.	·08	......	·56
13.	29·912	29·779	29·38	29·55	29·42	72	58	67	55	55	se.	w.	s.	·28	......	......
14. ☾	29·814	29·682	29·23	29·34	29·33	69	50	64	57	54	sw.	ssw.	w.	·02	·05	·49
15.	29·951	29·903	29·45	29·41	29·56	71	58	61	55	54	s.	sw.	sw.	·02	·03	·14
16.	29·876	29·822	29·30	29·35	29·41	71	62	69	57	58	sw.	sw.	sw.	·02	......	·18
17.	29·899	29·769	29·26	29·33	29·32	74	49	65	59	56	sw.	wsw.	sw.	......	......	......
18.	30·092	30·015	29·56	29·50	29·32	69	59	57·5	58	54	sw.	w.	sw.	......	·08	·22
19.	29·990	29·936	29·47	29·58	29·70	72	58	60·5	58	56	sw.	sw.	sw.	·05	·06	·25
20.	30·006	29·890	29·37	29·53	29·51	71	41	58	58	55	n.	nw.	w.	·02	·04	·06
21.	30·193	30·144	29·57	29·91	30·10	72	34	54	54	49	w.	w.	w.	·04	......	·26
22. ●	30·314	30·255	29·88	30·11	30·05	63	44	47	57	56	nw.	nw.	se.	......	......	......
23.	30·218	30·197	29·74	29·77	29·65	63	55	57	55	54	w.	nw.	nw.	·03	·02	·36
24.	30·093	30·058	29·50	29·58	29·68	70	36	62	46½	48½	w.	wsw.	wnw.	......	·02	·78
25.	30·300	30·247	29·80	29·99	30·04	64	34	51·5	49½	53½	n.	nw.	w.	......	......	·15
26.	30·342	30·304	29·90	29·97	30·00	69	34	50	57	55½	nw.	sw.	w.	......	......	·10
27.	30·332	30·219	29·90	30·09	30·09	70	33	51	59	55	se.	s.	sw.	......	......	......
28.	30·138	30·080	29·73	30·11	29·99	71	33	54	52½	53½	e.	se.	sw.	......	......	......
29. ☽	30·106	30·070	29·68	29·91	29·84	76	37	48	55½	54	ne.	nw.	sw.	......	......	......
30.	30·159	30·134	29·74	29·84	30·10	75	36	51	54½	52	n.	calm	sw.	·01	......	......
Mean.	30·163	30·098	29·67	29·879	29·881	71·33	42·53	57·8	55·93	54·21				0·58	0·30	4·18

LII. *On the Mechanical Energies of the Solar System.*
By Professor WILLIAM THOMSON [*].

THE mutual actions and motions of the heavenly bodies have long been regarded as the grandest phænomena of mechanical energy in nature. But their light has been seen, and their heat has been felt, without the slightest suspicion that we had thus a direct perception of mechanical energy at all. Even after it has been shown[†] that the almost inconceivably minute fraction of the sun's heat and light reaching the earth is the source of energy from which all the mechanical actions of organic life, and nearly every motion of inorganic nature at its surface, are derived, the energy of this source has been scarcely thought of as a development of mechanical power.

Little more than ten years ago the true relation of heat to force, in every electric, magnetic, and chemical action, as well as in the ordinary operations of mechanics, was pointed out[‡]; and it is a simple corollary from this that the sun, within the historical period of human observation, has emitted hundreds of

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* From the Transactions of the Royal Society of Edinburgh, vol. xxi. part 1; read April 17, 1854.

† Herschel's Astronomy, Edition 1833. See last Ed., § (399).

‡ Joule "On the Generation of Heat in the Galvanic Circuit," communicated to the Royal Society of London, Dec. 17, 1840, and published, Phil. Mag., Oct. 1841. "On the Heat evolved during the Electrolysis of Water," Literary and Phil. Soc. of Manchester, 1843, vol. vii. part 3, Second Series. "On the Calorific Effects of Magneto-Electricity, and the Mechanical Value of Heat," communicated to the British Association, August 1843, and published, Phil. Mag., Sept. 1843. "On the Changes of Temperature produced by the Rarefaction and Condensation of Air,"

times as much mechanical energy* as that of the motions of all the known planets taken together. The energy—that of light and radiant heat—thus emitted, is dissipated always more and more widely through endless space, and never has been, probably never can be, restored to the sun, without acts as much beyond the scope of human intelligence as a creation or annihilation of energy, or of matter itself, would be. Hence the question arises, What is the source of mechanical energy, drawn upon by the sun, in emitting heat, to be dissipated through space? In speculating on the answer, we may consider whether the source in question consists of dynamical energy, that is, energy of motion†, or of "potential energy" (as Mr. Rankine has called the energy of force acting between bodies which will give way to it unless held); or whether it consists partly of dynamical and partly of potential energy.

And again, we may consider whether the source in question, or any part of it, is in the sun, or exists in surrounding matter, until taken in and sent out again by the sun, or exists as energy only convertible into heat by mutual actions between the sun and surrounding matter.

If it be dynamical and entirely in the sun, it can only be primitive heat; if potential and in the sun, it can only be energy of chemical forces ready to act. If not in the sun, it must be due to matter coming to the sun (for it certainly is not a mere communication of motion to solar particles from external energy, as such could only be effected by undulations like sound or radiant heat, and we know that no such anti-radiation can be experienced by a body in the sun's circumstances); but whether intrinsically in such external matter, or developed by mutual action between this matter and the sun, and whether dynamical or potential in either case, requires careful consideration, as will be shown in the course of this communication. We see, then, that all the theories which have been yet proposed, as well as every conceivable theory, must be one or other, or a combination of the following three:—

I. That the sun is a heated body, losing heat.

II. That the heat emitted from the sun is due to chemical action among materials originally belonging to his mass, or that the sun is a great fire.

III. That meteors falling into the sun give rise to the heat which he emits.

communicated to the Royal Society, June 1844, and published, Phil. Mag., May 1845. Joule and Scoresby "On the Powers of Electro-magnetism, Steam, and Horses," Phil. Mag., June 1846.

* Once every twenty years or so. See Table of Mechanical Energies of the Solar System, appended.

† "Actual energy," as Mr. Rankine has called it.

In alluding to theories of solar heat in former communications to the Royal Society, I pointed out that the first hypothesis is quite untenable. In fact it is demonstrable, that, unless the sun be of matter inconceivably more conductive for heat, and less volatile, than any terrestrial meteoric matter we know, he would become dark in two or three minutes, or days, or months, or years, at his present rate of emission, if he had no source of energy to draw from but primitive heat*. The second has been not only held by the Fire-worshipers, but has probably been conceived of by all men in all times, and considered as more or less probable by every philosopher who has ever speculated on the subject. The third may have occurred at any time to ingenious minds, and may have occurred and been set aside as not worth considering; but was never brought forward in any definite form, so far as I am aware, until Mr. Waterston communicated to the British Association, during its meeting at Hull, a remarkable speculation on cosmical dynamics, in which he proposed the theory that solar heat is produced by the impact of meteors falling from extra-planetary space, and striking his surface with velocities which they have acquired by his attraction. This is a form of what may be called the Gravitation Theory of Solar Heat, which is itself included in the general meteoric theory.

The objects of the present communication are, to consider the relative capabilities of the second and third hypothesis to account for the phænomena; to examine the relation of the gravitation theory to the meteoric theory in general; and to determine what form of the gravitation theory is required to explain solar heat consistently with other astronomical phænomena.

In the first place it may be remarked, that in all probability there must always be meteors falling into the sun, since the fact of meteors coming to the earth† proves the existence of such bodies moving about in space; and even if the motions of these bodies are

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* This assertion is founded on the supposition that conduction is the only means by which heat could reach the sun's surface from the interior, and perhaps requires limitation. For it might be supposed that, as the sun is no doubt a melted mass, the brightness of his surface is constantly refreshed by incandescent fluid rushing from below to take the place of matter falling upon the surface after becoming somewhat cooled and consequently denser—a process which might go on for many years without any sensible loss of brightness. If we consider, however, the whole annual emission at the present actual rate, we find, even if the sun's thermal capacity were as great as that of an equal mass of water, that his mean temperature would be lowered by about 3° Cent. in two years. We may, I think, safely conclude that primitive heat within the sun is not a sufficient source for the emission which has continued without sensible (if any) abatement for 6000 years.—(May 4, 1854.)

† To make the argument perfectly conclusive, it would have to be assumed that meteors not only are, but have been, always falling to the

at any instant such as to correspond to elliptical or circular orbits round the sun, the effects of the resisting medium would gradually bring them in to strike his surface. Also, it is easy to prove dynamically that meteors falling in to the sun, whatever may have been their previous state of motion, must enter his atmosphere, or strike his surface, with, on the whole, immensely greater relative velocities than those with which meteors falling to the earth enter the earth's atmosphere, or strike the earth's surface. Now, Joule has shown what enormous quantities of heat must be generated from this relative motion in the case of meteors coming to the earth; and by his explanation* of "falling stars," has made it all but certain that, in a vast majority of cases, this generation of heat is so intense as to raise the body in temperature gradually up to an intense white heat, and cause it ultimately to burst into sparks in the air (and burn if it be of metallic iron) before it reaches the surface. Such effects must be experienced to an enormously greater degree before reaching his surface, by meteors falling to the sun, if, as is highly probable, he has a dense atmosphere; or they would take place yet more intensely on striking his solid or liquid surface, were they to reach it still possessing great velocities. Hence it is certain that *some* heat and light radiating from the sun is due to meteors. It is excessively probable that there is much more of this from any part of the sun's surface than from an equal area of the earth's, because of the enormously greater action that an equal amount of meteoric matter would produce in entering the sun, and because the sun, by his greater attraction, must draw in meteoric matter much more copiously with reference to equal areas of surface. We would have no right then, as was done till Mr. Waterston brought forward his theory, to neglect meteoric action in speculating on solar heat, unless we could prove, which we certainly cannot do, that its influence is insensible. It is in fact not only proved to exist as a cause of solar heat, but it is the only one of all conceivable causes which we know to exist from independent evidence.

To test the possibility of this being the *principal or the sole cause* of the phænomenon, let us estimate at what rate meteoric matter would have to fall on the sun, to generate as much heat

earth for some immense period of time. The conclusion, however, appears sufficiently probable with the facts we know.

* See Philosophical Magazine, May 1848, for reference to a lecture in Manchester, on the 28th April 1847, in which Mr. Joule said, that "the velocity of a meteoric stone is checked by the atmosphere and its *vis viva* converted into heat, which at last becomes so intense, as to melt the body and dissipate it in fragments too small probably to be noticed in their fall to the ground, in most cases."

as is emitted.   According to Pouillet's data*, ·06 of a thermal unit Centigrade is the amount of heat incident per second on a square foot directly exposed to solar radiation at the earth's distance from the sun, which being 95,000,000 miles, and the sun's radius being 441,000 miles, we infer that the rate of emission of heat from the sun is

$$\cdot 06 \times \left(\frac{95{,}000{,}000}{441{,}000} \right)^2 = 2781 \text{ thermal units per}$$

second per square foot of his surface.

The mechanical value of this (obtained by multiplying it by Joule's equivalent, 1390) is

$$83 \cdot 4 \times \left(\frac{95{,}000{,}000}{441{,}000} \right)^2 = 386{,}900 \text{ ft. lbs.}$$

Now if, as Mr. Waterston supposes, a meteor either strikes the sun, or enters an atmosphere where the luminous and thermal excitation takes place, *without having previously experienced any sensible resistance*, it may be shown dynamically (the velocity of rotation of the sun's surface, which at his equator is only a mile and a quarter per second, being neglected) that the least relative velocity which it can have is the velocity it would acquire by solar gravitation in falling from an infinite distance, which is equal to the velocity it would acquire by the action of a constant force equal to its weight at the sun's surface, operating through a space equal to his radius.   The force of gravity at the sun's surface being about 28 times that at the earth's surface, this velocity is

$$\sqrt{\frac{2 \times 28 \times 32 \cdot 2 \times 441{,}000}{5280}} = 390 \text{ miles per second; and}$$

its mechanical value per pound of meteoric matter is

$$28 \times 441{,}000 \times 5280 = 65{,}000{,}000{,}000 \text{ ft. lbs.}$$

Hence the quantity of meteoric matter that would be required, according to Mr. Waterston's form of the gravitation theory, to strike the sun per square foot is 0·000060 pound per second (or about a pound every five hours).   At this rate the surface would be covered to a depth of 30 feet in the year, if the density of the deposit is the same as that of water, which is a little less than the mean density of the sun†.   A greater rate of deposit than this could not be required, if the hypothesis of no resistance, except in the locality of resistance with luminous reaction, were true; but a less rate would suffice if, as is probable enough, the meteors in remote space had velocities relative

* *Mémoire sur la Chaleur Solaire*, &c., Paris 1838.   See *Comptes Rendus*, July 1838; or Pouillet, *Traité de Physique*, vol. ii.

† This is rather more than double the estimate Mr. Waterston has given. The velocity of impact which he has taken is 545 miles per second, in the calculation of which, unless I am mistaken, there must be some error.

to the sun not incomparably smaller than the velocity calculated above as due to solar gravitation.

But it appears to me that the hypothesis of no sensible resistance until the "sun's atmosphere" is reached, or the sun's surface struck, is not probable*; because if meteors were falling in to the sun in straight lines, or in parabolic or hyperbolic paths, in anything like sufficient quantities for generating all the heat he emits, the earth in crossing their paths would be, if not intolerably pelted, at least struck much more copiously by meteors than we can believe it to be from what we observe; and because the meteors we see appear to come generally in directions corresponding to motions which have been elliptic or circular, and rarely if ever in such directions as could correspond to previous parabolic, hyperbolic, or rectilineal paths towards the sun.   If this opinion and the first mentioned reason for it be correct, the meteors containing the stores of energy for future sun-light must be principally within the earth's orbit : and we actually see them there as the "zodiacal light," an illuminated shower or rather tornado of stones (Herschel, § 897).   The inner parts of this tornado are always getting caught in the sun's atmosphere, and drawn to his mass by gravitation.   The bodies in all parts of it, in consequence of the same actions, must be approaching the sun, although but very gradually; yet, in consequence of their comparative minuteness, much more rapidly than the planets. The outer edge of the zodiacal light appears to reach to near the earth at present (Herschel, § 897); and in past times it may be that the earth has been in a dense enough part of it to be kept hot, just as the sun is now, by drawing in meteors to its surface.

According to this form of the gravitation theory, a meteor would approach the sun by a very gradual spiral, moving with a velocity very little more than that corresponding to a circular path at the same distance, until it begins to be much more resisted, and to be consequently rapidly deflected towards the sun; then the phænomenon of ignition commences; after a few seconds of time all the dynamical energy the body had at the commencement of the sudden change is converted into heat and radiated off; and the mass itself settles incorporated in the sun. It appears, therefore, that the velocity which a meteor loses in entering the sun is that of a satellite at his surface, which (being $\frac{1}{\sqrt{2}}$ of that due to gravitation from an infinite distance) is 276 miles per second.   The mechanical value (being half that of a body falling to the sun from a state of comparatively slow motion in space) is about 32,500,000,000 ft. lb. per pound of meteoric matter; hence the fall of meteors must be just twice

* For a demonstration that it is not possible, see Addition No. 1.

that which was determined above according to Mr. Waterston's form of the theory, and must consequently amount to 3800 lbs. annually per square foot. If, as was before supposed, the density of the deposit is the same as that of water, the whole surface would be covered annually to a depth of 60 feet, from which the sun would grow in diameter by a mile in 88 years. It would take 4000 years at this rate to grow a tenth of a second in apparent diameter, which could scarcely be perceived by the most refined of modern observations, or 40,000 years to grow 1″, which would be utterly insensible by any kind of observation (that of eclipses included) unassisted by powerful telescopes. We may be confident, then, that the gradual augmentation of the sun's bulk required by the meteoric theory to account for this heat, may have been going on in time past during the whole existence of the human race, and yet could not possibly have been discovered by observation, and that at the same rate it may go on for thousands of years yet without being discoverable by the most refined observations of modern astronomy. It would take, always at the same rate, about 2,000,000 years for the sun to grow in reality as much as he appears to grow from June to December by the variation of the earth's distance, which is quite imperceptible to ordinary observation. This leaves for the speculations of geologists on ancient natural history a wide enough range of time with a sun not sensibly less than our present luminary: still more, the meteoric theory affords the simplest possible explanation of past changes of climate on the earth. For a time the earth may have been kept melted by the heat of meteors striking it. A period may have followed when the earth was not too hot for vegetation, but was still kept, by the heat of meteors falling through its atmosphere, at a much higher temperature than at present, and illuminated in all regions, polar as well as equatorial, before the existence of night and day. Lastly; although a very little smaller, the sun may have been at some remote period much hotter than at present by having a more copious meteoric supply.

A dark body of dimensions such as the sun, in any part of space, might, by entering a cloud of meteors, become incandescent as intensely in a few seconds as it could in years of continuance of the same meteoric circumstances ; and on again getting to a position in space comparatively free from meteors, it might almost as suddenly become dark again. It is far from improbable that this is the explanation of the appearance and disappearance of bright stars, and of the strange variations of brilliancy of others which have caused so much astonishment*.

* The star which Mr. Hind discovered in April 1848, and which only remained visible for a few weeks, during which period it varied considerably

The amount of matter, drawn by the sun in any time from surrounding space, would be such as in $47\frac{1}{2}$ years to amount to a mass equal to that of the earth.   Now there is no reason whatever to suppose that 100 times the earth's mass drawn in to the sun, would be missed from the zodiacal light (or from meteors revolving inside the orbit of Mercury, whether visible as the "zodiacal light" or not) ; and we may conclude that there is no difficulty whatever in accounting for a constancy of solar heat during 5000 years of time past or to come.   Even physical astronomy can raise no objection by showing that the sun's mass has not experienced such an augmentation ; for according to the form of the gravitation theory which I have proposed, the added matter is drawn from a space where it acts on the planets with very nearly the same forces as when incorporated in the sun. This form of the gravitation theory then, which may be proved to require a greater mass of meteoric matter to produce the solar heat than would be required on any other assumption that could be made regarding the previous positions and motions of the meteors, requires not more than it is perfectly possible does fall in to the sun.   Hence I think we may regard the adequacy of the meteoric theory to be fully established.

Let us now consider how much chemical action would be required to produce the same effects, with a view both to test the adequacy of the theory that the sun is merely a burning mass without a supply of either fuel or dynamical energy from without, and to ascertain the extent to which, in the third theory, the combustion of meteors may contribute, along with their dynamical energies, to the supply of solar heat.   Taking the former estimate, 2781 thermal units Centigrade, or 3,869,000 foot-lbs. as the rate per second of emission of energy from a square foot of the sun's surface, equivalent to 7000 horse-power, we find that more than ·42 of a lb. of coal per second, or 1500 lbs. per hour would be required to produce heat at the same rate.   Now if all the fires of the whole Baltic fleet were heaped up and kept in full combustion, over one or two square yards of surface, and if the surface of a globe all round had every square yard so occupied, where could a sufficient supply of air come from to sustain the combustion ? yet such is the condition we must suppose the sun to be in, according to the hypothesis now under consideration, at least if one of the combining elements be oxygen or any other gas drawn from the surrounding atmosphere.   If the products of combustion were gaseous, they would in rising check the necessary supply of fresh air ; or if they be solid or

in appearance and brightness, but was always of a "ruddy" colour, may have not experienced meteoric impact enough to make its surface more than red-hot.

liquid (as they might be wholly or partly if the fuel be metallic) they would interfere with the supply of the elements from below. In either or in both ways the fire would be choked, and I think it may be safely affirmed that no such fire could keep alight for more than a few minutes, by any conceivable adaptation of air and fuel. If then the sun be a burning mass, it must be more analogous to burning gunpowder than to a fire burning in air; and it is quite conceivable that a solid mass, containing within itself all the elements required for combustion, *provided the products of combustion are permanently gaseous**, could burn off at its surface all round, and actually emit heat as copiously as the sun. Thus an enormous globe of gun-cotton might, if at first cold, and once set on fire round its surface, get to a permanent rate of burning, in which any internal part would become heated by conduction, sufficiently to ignite only when nearly approached by the diminishing surface. It is highly probable indeed that such a body might for a time be as large as the sun, and give out luminous heat as copiously, to be freely radiated into space, without suffering more absorption from its atmosphere of transparent gaseous products† than the light of the sun actually does experience from the dense atmosphere through which it passes. Let us therefore consider at what rate such a body, giving out heat so copiously, would diminish by burning away. The heat of combustion could probably not be so much as 4000 thermal units per pound of matter burned‡, the greatest thermal equivalent of chemical action yet ascertained falling considerably short of this. But 2781 thermal units (as found above) are emitted per second from each square foot of the sun; hence there would be a loss of about ·7 of a pound of matter per square foot per second. Such a loss of matter from every square foot, if of the mean density of the sun (a little more than that of water), would take off from the mass a layer of about ·5 of a foot thick in a minute, or of about 55 miles thick in a year. At the same rate continued, a mass as large as the sun is at present would burn away in 8000 years. If the sun has been burning at that rate in past time, he must have been of double diameter, of quadruple heating power, and of eight-fold mass, only 8000 years ago. We may quite safely conclude then that the sun does not get its heat by chemical action among particles of matter primitively belonging to his own mass, and we must therefore look to the meteoric theory for fuel, even if we retain the idea of a fire. Now, according to Andrews, the heat of

* On this account gunpowder would not do.
† These would rise and be regularly diffused into space.
‡ Both the elements that enter into combination are of course included in the weight of the burning matter.

combustion of a pound of iron in oxygen gas is 1301 thermal units, and of a pound of potassium in chlorine 2655; a pound of potassium in oxygen 1700 according to Joule; and carbon in oxygen, according to various observers, 8000. The greatest of these numbers, multiplied by 1390 to reduce to foot-pounds, expresses only the 6000dth part, according to Mr. Waterston's theory, and, according to the form of the gravitation theory now proposed, only the 3000dth part, of the least amount of dynamical energy a meteor can have on entering the region of ignition in the sun's atmosphere. Hence a mass of carbon entering the sun's atmosphere, and there burning with oxygen, could only by combustion give out heat equal to the 3000dth part of the heat it cannot but give out from its motion. Probably no kind of known matter (and no meteors reaching the earth have yet brought us decidedly new elements) entering the sun's atmosphere from space, whatever may be its chemical nature, and whatever its dynamical antecedents, could emit by combustion as much as $\frac{1}{1000}$dth of the heat inevitably generated from its motion. It is highly probable that many, if not all, meteors entering the sun's atmosphere do burn, or enter into some chemical combination with substances which they meet. Probably meteoric iron comes to the sun in enormous quantities, and burns in his atmosphere, just as it does to the earth. But (while probably nearly all the heat and light of the sparks which fly from a steel struck by a flint is due to combustion alone) only $\frac{1}{18000}$dth part of the heat and light of a mass of iron entering the sun's atmosphere, or $\frac{1}{3}$th of the heat and light of such a meteor entering our own, can possibly be due to combustion. Hence the combustion of meteors may be quite disregarded as a source of solar heat.

At the commencement of this communication, it was shown that the heat radiated from the sun is either taken from a stock of primitive solar heat, or generated by chemical action among materials originally belonging to his mass, or due to meteors falling in from surrounding space. We saw that there are sufficient reasons for utterly rejecting the first hypothesis; we have now proved that the second is untenable; and we may consequently conclude that the third is true, or that meteors falling in from space give rise to the heat which is continually radiated off by the sun. We have also seen that no appreciable portion of the heat thus produced is due to chemical action, either between the meteors and substances which they meet at the sun, or among elements of the meteors themselves; and that whatever may have been their original positions or motions relatively to one another or to the sun, the greater part of them fall in gradually from a state of approximately circular motion, and

strike the sun with the velocity due to half the potential energy of gravitation lost in coming in from an infinite distance to his surface. The other half of this energy goes to generate heat very slowly and diffusely in the resisting medium. Many a meteor, however, we cannot doubt, comes in to the sun at once in the course of a rectilineal or hyperbolic path, without having spent any appreciable energy in the resisting medium; and, consequently, enters the region of ignition at his surface with a velocity due to the descent from its previous state of motion or rest, and there converts both the dynamical effect of the potential energy of gravitation, and the energy of its previous motion, if it had any, into heat, which is instantly radiated off to space. But the reasons stated above make it improbable that more than a very small fraction of the whole solar heat is obtained by meteors coming in thus directly from extra-planetary space.

In conclusion, then, the source of energy from which solar heat is derived is undoubtedly meteoric. It is not any intrinsic energy in the meteors themselves, either potential, as of mutual gravitation or chemical affinities among their elements; or actual, as of relative motions among them. It is altogether dependent on mutual relations between those bodies and the sun. A portion of it, although very probably not an appreciable portion, is that of motions relative to the sun, and of independent origin. The principal source, perhaps the sole appreciably efficient source, is in bodies circulating round the sun at present inside the earth's orbit, and probably seen in the sunlight by us and called "the zodiacal light." The store of energy for future sunlight is at present partly dynamical, that of the motions of these bodies round the sun; and partly potential, that of their gravitation towards the sun. This latter is gradually being spent, half against the resisting medium, and half in causing a continuous increase of the former. Each meteor thus goes on moving faster and faster, and getting nearer and nearer the centre, until some time, very suddenly, it gets so much entangled in the solar atmosphere as to begin to lose velocity. In a few seconds more it is at rest on the sun's surface, and the energy given up is vibrated in a minute or two across the district where it was gathered during so many ages, ultimately to penetrate as light the remotest regions of space.

Explanation of Tables.

The following tables exhibit the principal numerical data regarding the mechanical energies of the solar system.

In Table I., the mass of the earth is estimated on the assumption that its mean density is five times that of water, and the other

masses are shown in their true proportions to that of the earth, according to data which Professor Piazzi Smyth has kindly communicated to the author.

In Table II., the mechanical values of the rotations of the sun and earth are computed on the hypothesis, that the moment of inertia of each sphere is equal to the square of its radius multiplied by only one-third of its mass, instead of two-fifths of its mass, as would be the case if its matter were of uniform density. These two estimates are only introduced for the sake of comparison with other mechanical values shown in the table, not having been used in the reasoning.

The numbers in the last column of Table II., showing the times during which the sun emits quantities of heat mechanically equivalent to the earth's motion in its orbit, and to its motion of rotation, were first communicated to the Royal Society on the 9th of January 1852, in a paper "On the Sources Available to Man for the production of Mechanical Effect." These, and the other numbers in the same column, are the only part of the numerical data either shown in the tables, or used directly or indirectly in the reasoning on which the present theory is founded, that can possibly require any considerable correction; depending as they do on M. Pouillet's estimate of solar heat in thermal units. The extreme difficulties in the way of arriving at this estimate, notwithstanding the remarkably able manner in which they have been met, necessarily leave much uncertainty as to the degree of accuracy of the result. But even if it were two or three times too great or too small (and there appears no possibility that it can be so far from the truth), the general reasoning by which the theory of solar heat at present communicated is supported, would hold with scarcely altered force.

The mechanical equivalent of the thermic unit, by which the solar radiation has been reduced to mechanical units, is Mr. Joule's result—1390 foot-pounds for the thermal unit Centigrade—which he determined by direct experiment with so much accuracy, that any correction it may be found to require can scarcely amount to $\frac{1}{200}$ or $\frac{1}{300}$dth of its own value.

Table I.—Forces and Motions in the Solar System.

	Masses in pounds.	Distances from the suns's centre, in miles.	Forces of attraction towards the sun, in terrestrial pounds.	Velocities, in miles per second.
Sun..............	4,230,000,000×10²¹	(surface) 441,000	28·61 per lb. of matter	(equator) 1·27
Imaginary solid planet close to the sun	1×20²¹	441,000	286,100×10¹⁷	277
Mercury........	870×10²¹	36,800,000	35,710×10¹⁷	30·36
Venus	10,530×10²¹	68,700,000	124,200×10¹⁷	22·22
Earth	11,920×10²¹	95,000,000	73,490×10¹⁷	18·89
Mars	1,579×10²¹	144,800,000	4,211×10¹⁷	15·28
Jupiter	4,037,000×10²¹	494,300,000	919,400×10¹⁷	8·28
Saturn	1,208,000×10²¹	906,200,000	81,855×10¹⁷	6·11
Uranus	201,490×10²¹	1,822,000,000	3,377×10¹⁷	4·31
Neptune	236,380×10²¹	2,854,000,000	1,615×10¹⁷	3·44
		Distances from earth's centre.	Attraction towards earth in terrestrial pounds.	Velocities relatively to earth's centre, in miles.
Moon	136×10²¹	237,000	378×10¹⁷	0·615
Earth's equator.	....	3,956	1 per lb. of matter.	0·291

Table II.—Mechanical Energies of the Solar System.

	Potential energy of gravitation to sun's surface.		Actual energy relatively to sun's centre.	
	In foot-pounds.	Equivalent to supply of solar heat, at the present rate of radiation, for a period of	In foot-pounds.	Equivalent to supply of solar heat, at the present rate of radiation, for a period of
Sun..................	....	....	967,000×10³⁰	116 yrs. 6 days.
Imaginary planet of 10²¹ lbs. of matter, close to the sun	....	....	333×10²⁹	1·44 ...
Mercury	57×10³³	6 yrs. 214 days	347×10³⁰	15·2 ...
Venus...............	697×10³³	83 ... 227 ...	2,252×10³⁰	98·5 ...
Earth	790×10³³	94 ... 303 ...	1,843×10³⁰	80·7 ...
Mars	105×10³³	12 ... 252 ...	160×10³⁰	7·0 ...
Jupiter	268,800×10³³	32,240	119,980×10³⁰	14yrs.144 ...
Saturn	80,440×10³³	9,650	19,580×10³⁰	2 ... 127 ...
Uranus	13,430×10³³	1,610	1,625×10³⁰	71·2 ...
Neptune............	15,750×10³³	1,890	1,217×10³⁰	53·3 ...
	To the earth's surface.		Relatively to earth's centre.	
Moon	2,846×10²⁷	3·0 hours.	2,347×10²⁵	1·48 min.
Earth (rotation)...	....	....	14,310×10²⁵	9·03 ...
Total	380,000×10³³	45,589 years.	1,114,004×10³⁰	134 years.

ADDITIONS (May 9, 1854), No I.   *Conclusion of Physical Astro-
nomy against the Extra-planetary Meteoric Theory.*

Meteors which when at great distances possessed, relatively to
the centre of gravity of the solar system, velocities not incom-
parably smaller than the velocity due to gravitation to the sun's
surface, must strike the surfaces of the earth and of the other
planets not incomparably less frequently than equal areas of the
sun's surface, and with not incomparably smaller velocities, and
consequently must generate heat at the surfaces of the earth and
other planets not incomparably less copiously than at equal areas
of the sun's surface.   But the whole heat emitted from any part
of the sun's surface is incomparably greater than all that is gene-
rated by meteors on an equal area of the earth's surface, and
therefore is incomparably greater than all that can be generated
at his own surface by meteors coming in with velocities exceeding
considerably the velocity due to his attraction from an infinite
distance.   Hence upon the extra-planetary meteoric theory of
solar heat the quantity of matter required to fall in cannot be
much, if at all, less than that required upon the hypothesis that
the work done by the sun's attraction is equal to the mechanical
value of the heat emitted from his surface, and must therefore
be, as found above, about ·000060 of a pound per square foot
per second, or 1900 lbs. per square foot in a year.   The mean
density of the sun being about $1\frac{1}{4}$ times that of water, the matter
in a pyramidal portion from his centre to a square foot of his
surface is about

$$\tfrac{1}{3} \times 441,000 \times 5280 \times 1\tfrac{1}{4} \times 64 = 62,100,000,000 \text{ lbs.},$$

and the whole annual addition of meteoric matter to the sun
would therefore be

$$\frac{1900}{62,100,000,000} = \frac{1}{32,400,000}$$

of his own mass.   In about 6000 years the sun would there-
fore be augmented by $\frac{1}{5000}$ in mass from extra-planetary space.
Since the time occupied by each meteor in falling to the sun
from any distance would be much less than the periodic time of
a planet revolving at that distance, and since the periodic times
of the most distant of the planets is but a small fraction of 6000
years, it follows that the chief effect on the motions of the planetary
system produced during such a period by the attraction of the
matter falling in would be that depending simply on the aug-
mentation of the central force.   To determine this, let M be the
sun's mass at any time t, measured from an epoch 6000 years
ago; ω the earth's mean angular velocity, and a its mean

distance at the same time; and $2h$ the constant area described by its radius vector per second.   Then we have—

$$\omega^2 a = \frac{M}{a^2} \text{ (centrifugal force)},$$

$$\omega a^2 = h \text{ (equable description of areas)};$$

from which we deduce,

$$a = \frac{h^2}{M},$$

and

$$\omega = \frac{M^2}{h^3}.$$

Now, if M_0 denote the mass of the sun at the epoch from which time is reckoned, since the annual augmentation is about $\frac{1}{32400000}$ of the mass itself, we have

$$M = M_0 \left(1 + \frac{t}{32,400,000} \right),$$

and

$$M^2 = M_0{}^2 \left(1 + \frac{2t}{32,400,000} \right).$$

Hence, if Ω_0 and Ω_T denote the angular velocities at the epoch and at the present time, T; the angular velocity, which is uniformly accelerated during the interval, will have a mean value, Ω, expressed as follows:—

$$\Omega = \tfrac{1}{2} \Big(\Omega_0 + \Omega_T \Big) = \Omega_T \left\{ 1 - \tfrac{1}{2} \frac{\Omega_T - \Omega_0}{\Omega_T} \right\} = \Omega_T \left(1 - \frac{T}{32,400,000} \right);$$

and if Θ denote the angle described in the time T, we have

$$\Theta = \Omega_T \left(T - \frac{T^2}{32,400,000} \right).$$

To test this conclusion for the case of the earth, let T' denote the number of revolutions round the sun in the time T.   Then, if the unit in which T is measured be the time of a revolution with the angular velocity Ω_T, we have

$$T' = T - \frac{T^2}{32,400,000}.$$

Thus, if T be 4000 years, we have

$$T' = 4000 - \frac{16,000,000}{32,400,000} = 3999\tfrac{1}{2};$$

or only $3999\tfrac{1}{2}$ actual years in a period of 4000 times the present

year. Similarly, we should find a loss of $\frac{1}{6}$th of a year on a period of 2000 years ago; that is, of about a month and a half since the Christian æra. Thus, if we reckon back about 2000 times the number of days at present in the year, we should find seasons, new and full moons, and eclipses, a month and a half later than would be if the year had been constantly what it is. Now we have abundant historical evidence that there is no such dislocation as this, either in the seasons, or in the lunar phænomena; and it follows that the central attracting mass of the solar system does not receive the augmentation required by the extra-planetary meteoric theory of solar heat. But the reasoning in the preceding paper establishes, with very great probability, a meteoric theory of solar heat; and we may therefore conclude that the meteors supplying the sun with heat have been for thousands of years far within the earth's orbit.

No. II. *Friction between Vortices of Meteoric Vapour and the Sun's Atmosphere the immediate Cause of Solar Heat.*

It has been shown that the meteors which contribute the energy for solar heat must be for thousands of years within the earth's orbit before falling to the sun. But a meteor could not remain for half a year there, unless it were revolving round the sun, with, at each instant, the elements of a circular or elliptic orbit. Hence meteors, on their way in to the sun, must revolve, each, thousands of times round him, in orbits which, whatever may have been their primitive eccentricities, must tend to become more and more nearly circular as they become smaller, by the effects of the resisting medium. The resistance must be excessively small, even very near the sun; since a body of such tenuity as a comet, darting at the rate of 365 miles per second, within one-seventh of his radius from his surface, comes away without sensible loss of energy. If, as is probable, the atmosphere of that part of space is carried in a *vortex* round the sun by the meteors and other planets, it may be revolving at nearly the same rates as these bodies at different distances in the principal plane of the solar system; but we cannot conceive it to be revolving in any locality more rapidly than a planet at the same distance. At one-seventh of the sun's radius from his surface, this would be about 258 miles per second; and therefore a comet approaching so near the sun, could not have a less velocity relatively to the resisting medium than 107 miles per second, and, if going against the stream, might have as great a relative velocity as 623 miles. On the other hand, the great body of the meteors circulating round the sun, and carrying the resisting medium along with them, may be moving through it with but

small relative velocities; the smaller for each individual meteor, the smaller its dimensions. The effects of the resistance must therefore be very gradual in bringing the meteors in to the sun, even when they are very near his surface; and we cannot tell how many years, or centuries, or thousands of years, each meteor, according to its dimensions, might revolve within a fraction of the sun's radius from his surface, before falling in, if it continued solid; but we may be sure that it would so revolve long enough to take, in its outer parts at least, nearly the temperature of that portion of space; and therefore, probably, unless it be of some substance infinitely less volatile than any terrestrial or meteoric matter known to us, long enough to be wholly converted into vapour (the mere fact of a comet* escaping from so near the sun as has been stated, being enough to show that there is, at such a distance, no sufficient atmospheric pressure to prevent evaporation with so high a temperature). Even the planet Mercury, if the sun is still bright when it falls in, will, in all probability, be dissipated in vapour long before it reaches the region of intense resistance, instead of (as it would inevitably do if not volatile) falling in solid, and in a very short time (perhaps a few seconds) generating three years' heat, to be radiated off in a flash which would certainly scorch one-half of the earth's surface, or perhaps the whole, as we do not know that such an extensive disturbance of the luminiferous medium would be confined by the law of rectilineal propagation. Each meteor, when volatilized, will contribute the actual energy it had before evaporation to a vortex of revolving vapours, approaching the sun spirally to supply the place of the inner parts, which, from moving with enormously greater velocities than the parts of the sun's surface near them, first lose motion by intense resistance, emitting an equivalent of radiant heat and light, and then, from want of centrifugal force, fall into the sun, and consequently become condensed to a liquid or solid state at his surface, where they settle. The latent heat absorbed by the meteors in evaporation, and afterwards partially emitted in their condensation at a higher temperature, is probably as insensible, in comparison with the heat of friction, as it has been shown the heat of any

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* That a comet may escape with only a slight loss by evaporation, if the resistance is not too great to allow it to escape at all, is easily understood, when we consider that it cannot be for more than a few hours exposed to very intense heat (not more than two or three hours within a distance equal to the sun's radius from his surface). If it consist of a cloud of solid meteors, the smallest fragments may be wholly evaporated immediately; but all whose dimensions exceed some very moderate limit of a few feet would, unless kept back by the resisting medium and made to circulate round the sun until evaporated, get away with only a little boiled off from their surfaces.

combustion or chemical action they can experience must be, or, as we have tacitly assumed the heat is which is taken and kept by the meteors themselves in approaching from cold space to lodge permanently in the sun. We may conclude that the sun's heat is caused, not by solids striking him, or darting through his atmosphere, but by friction in an atmosphere of evaporated meteors, drawn in and condensed by gravitation while brought to rest by the resistance of the sun's surface. The quantity of meteoric matter required, if falling in solid, would, as we have seen, be such that half the work done by solar gravitation on it, in coming from an infinite distance, is equal to the energy of heat emitted from the sun, and would therefore amount to a pound every 2·3 hours per square foot of the sun's surface; and it will be the same as this, notwithstanding the process of evaporation and condensation actually going on, if, as appears probable enough, the velocity of the vortex of vapour immediately external to the region of intense resistance in all latitudes be nearly equal to that of a planet close to the sun.

No. III. *On the Distribution of Temperature over the Sun's Surface.*

Not only the larger planets, but the great mass of meteors revolving round the sun, appear to revolve in planes nearly coinciding with his equator, and therefore such bodies, if solid when drawn in to the sun, would strike him principally in his equatorial regions, and would cause so much a more copious radiation of heat from those regions than from any other parts of his surface, that the appearance would probably be a line or band of light, instead of the round bright disc which we see. The nearly uniform radiation which actually takes place from different parts of the sun's surface appears to be sufficiently accounted for by the distillation of meteors, which we have seen must, in all probability, take place from an external region of evaporation at a considerable distance (perhaps several times his radius) inwards to his surface where they are condensed. Whatever be the dynamical condition of the luminous atmosphere of intense resistance, it is clear that there must be a very strong tendency to an equality of atmospheric pressure over the probably liquid surface of the sun, and that the temperature of the surface must be everywhere kept near that of the physical equilibrium between the vapours and the liquid or solid into which they are distilling. A lowering of temperature in any part would therefore immediately increase the rate of condensation of vapour into it, and so bring a more copious influx of meteoric matter with dynamical energy to supply the deficiency of heat. The various deviations from uniformity which have been observed in

the sun's disc are probably due to eddies which must be continually produced throughout the atmosphere of intense resistance between his surface (which at the equator revolves only at the rate of 1·3 mile per second) and the great vortex of meteoric vapour, which a few miles outside revolves at the rate of 277 miles per second about the equatorial regions, and (if not at the same) certainly at enormously great rates a few miles from the sun's surface in other localities. Such eddies may ordinarily be seen as the streaks which have been compared to "the streamers of our northern lights" (Herschel, § 387), and when any one of them sends a root down to the sun's surface it may cause one of the "minute dark dots or pores" which have been observed, and which, when attentively watched, are found to be always changing in appearance (Herschel). A great rotatory storm, like the tropical hurricanes in the earth's atmosphere, may occasionally result from smaller eddies accidentally combining, or from some disturbing cause originating at once an eddy on a much larger scale than usual, and may traverse the sun's surface, preventing the distillation of meteoric vapour over a great area, and consequently checking both the supply of dynamical energy for radiant heat in the luminous atmosphere of resistance, and the torrents of condensed meteoric vapours falling to the surface below it. The consequence would be, that the meteoric rain (Herschel's "cloudy stratum") would be cleared away for a certain space under the central parts of the storm by falling down to the liquid or solid surface, and the luminous atmosphere would lose intensity over a larger space bounded very irregularly by a region of minor eddies, which would cause varying streaks of light. These are exactly the circumstances assumed by Sir William Herschel to account for the great spots with their dark centres surrounded by sharply terminated penumbræ inside the abrupt ragged boundaries of the bright surface, and the branching luminous streaks or "faculæ" in the bright surface outside in their neighbourhood. (Sir John Herschel's Astronomy, § 388.)

No. IV. (Added August 15, 1854.) *On the Age of the Sun.*

The moment of the sun's rotatory motion (according to the hypothesis mentioned above in the "Explanation of the Tables" regarding the moment of inertia of his mass) is one-third of his mass multiplied by his radius, multiplied by the linear velocity of his equator, and is therefore equal to that of a planet at his surface having a mass equal to $\dfrac{1\cdot27}{3\times277}=\dfrac{1}{650}$ of his own mass. This is equal to the quantity of meteoric matter which would fall in during 25,000 years, at the present rate; and therefore

25,000 years is the time the sun would take to acquire his actual motion of rotation, by the incorporation of meteors, if these bodies were each revolving in the plane of his equator immediately before entering the region of intense resistance. But it has been shown to be probable that a great space round the sun is occupied by a vortex of evaporated meteors, and that the incorporation of meteoric matter takes place in reality by the condensation of vapour in a stratum close to his surface all round. It appears not improbable that the tangential velocity of this vortex immediately external to the radiant region of intense resistance may be found to be, in all solar latitudes, very nearly that of a planet close to the sun. If it be so, the moment of the motion communicated to the sun by any mass of meteoric matter will be $\dfrac{3\cdot14159\ldots\ldots}{4}$ of what would be communicated by the incorporation of an equatorial planet of equal mass: as much as $\frac{1}{510}$ of the sun's mass would have to fall in to produce his present rotation: and 32,000 years would be the time in which this would take place, at the present rate of meteoric incorporation as estimated above.

It will be a very interesting hydrodynamical problem to fully investigate the motion of the meteoric vortex; and among results to be derived from it will be strict estimates of the contribution to the sun's rotatory motion, and of the quantity of heat generated, by any amount of meteoric matter in becoming incorporated. With these, and with an accurate determination of the rate at which the sun radiates heat, we should be able to fix with certainty the augmentation of his velocity of rotation actually taking place at present from year to year, and to estimate the time during which the existing rotation would be acquired by meteoric incorporation going on always at the present rate and in the present manner. Whatever this time (which I shall call T years, to avoid circumlocution below) may be, probably will not be found to differ very widely from the preceding estimate of 32,000 years.

Now, from the fact that the sun's equator, the planets' orbits, and the zodiacal light, all lie nearly in one plane, it appears highly probable that the sun's present motion has really been acquired by the incorporation of meteors. It is certain that the present manner and rate of meteoric action cannot have been going on for more than the indicated period (T), without giving the sun a greater rotatory motion than he has, unless (which is very improbable) he were previously rotating in a contrary direction round the same axis: and, at only the present rate, it cannot have been going on for less than that time, unless the sun has been created with a rotatory motion round his present

axis, or has acquired such a motion from some independent mechanical action. The actual rate of solar radiation in time past may, for all we know, have been sometimes much greater and sometimes much less than at present; and there probably has been a time before, when meteors in abundance fell direct to the sun from extra-planetary space, some getting stopped on their way by the earth, and illuminating it by friction in its atmosphere and impact at its surface. But the kind of meteoric action now going on, has in all probability produced neither more nor less than T times the quantity of heat now emitted from the sun in one year. All things considered, it seems not improbable that the earth has been efficiently illuminated by the sun alone for not many times more or less than 32,000 years.

As for the future, it will be a most interesting problem to determine the mass of the zodiacal light (that is, matter external to the sun's mass, and within the earth's orbit), by the perturbations it may probably enough be discovered to produce in the motions of the visible planets. It could scarcely, I think, amount to $\frac{1}{x}$ th of the sun's mass (probably not to nearly as much), without producing such perturbations as could not have been overlooked in the present state of astronomical science; and we have seen that meteors amounting to $\frac{1}{5000}$ dth of the sun's mass, must, at the present estimated rate, fall in in 3000 years. I conclude that sunlight cannot last as at present for 300,000 years.

The continual acceleration of the sun's rotatory motion, which the preceding theory indicates, must, sooner or later, be tested by direct observation. The rate of acceleration (which for many thousands of years past and to come must remain sensibly constant, if the solar radiation continues so) is such that the angular velocity is increased annually by $\frac{1}{T}$ of its present value.

If T be 32,000, according to the preceding conjectural estimate, the effect in fifty-three years would amount to diminishing the period of the sun's revolution by an hour; and the actual effect cannot, according to the theory, be incomparably greater or less. It is just possible that a careful comparison of early with recent observations on the apparent motions of the dark spots may demonstrate this variation; but as some of the most accurate of recent observations of this kind have led to estimates of the period of revolution* differing from one another by as much as

	Days.	Hours.	Minutes.
* According to Böhm	25	12	30
,, Laugier	25	8	10
,, Petersen	25	4	30

(See Encyc. Brit., 8th edit. vol. iv. p. 87.) The discrepancies are probably due to proper motions of the spots, which, from the explanation given above in Addition III., may be expected to be very considerable.

eight hours, it is more probable that, unless some way be discovered for taking into account the motions of the spots themselves with reference to the mass, centuries will elapse before direct evidence can be had either for or against the anticipated acceleration of the sun's rotatory motion.

LIII. *On a Method of obtaining rapid Adjustments with* Wollaston's *Goniometer.* *By* C. Greville Williams[*].

CHEMISTS who have been in the habit of using Wollaston's goniometer are aware of the trouble and sometimes difficulty of making each face of the crystal so reflect the black lines, that, on moving the instrument until both approximate, they shall be truly parallel, and at length, as the movement is continued, perfectly coincide.

Most operators find it difficult, in moving the crystal with the fingers on the wax, to prevent disturbing the first adjustment while making the second. With some crystals this is comparatively easy, with others great loss of time is occasioned by it. The fact is, that the movements of the fingers are too coarse, and it occurred to me that it would be extremely easy to make an instrument that should possess one motion for one face of the crystal, and a second for the other.

Fig. 1 represents the contrivance I have adopted before being attached to the goniometer, fig. 2 shows it in its place.

It will be seen that any chemist can construct it with the tools to be found in every laboratory.

Fig. 1.

A small ring of brass is pierced with two grooves exactly opposite each other, through which passes an axle supporting in its centre a small ball, which in its turn has a hole drilled through it, and tapped so that a vertical screw carrying two milled heads can be inserted. One of these milled heads supports the crystal, the other serves to move it, and thereby communicate a horizontal motion. A lateral motion is obtained by the horizontal axle, which enables it to be moved to the left or right. The whole system is screwed into the small brass plate usually used to support the crystal; the entire arrangement is seen in fig. 2. The pivot in fig. 1 is made to move stiffly by means of two small plates screwed tight on to the grooves in which it works; moreover, from the vertical rod being formed from a screw, it can be depressed so as to make the face of the crystal lie exactly in the axis of the instrument.

* Communicated by the Author.

Fig. 2.
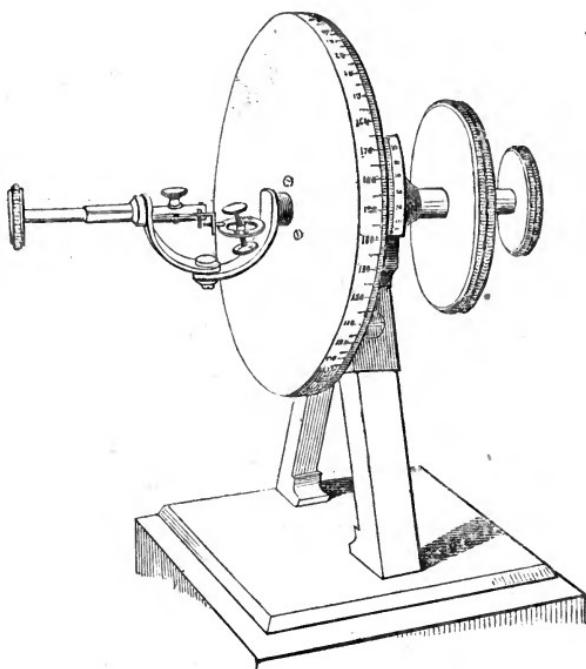
By the use of this little contrivance, perfect adjustments which would otherwise require a very considerable time, may be made in a few seconds.

LIV. *On a General Method in the Theory of Probabilities. By* GEORGE BOOLE, *LL.D., Professor of Mathematics in Queen's College, Cork*[*].

Preliminary Statement of Principles.

1. THE class of questions which I propose to consider here is that of which the data consist of—

1st. Probabilities of events, or of combinations of events, capable of being expressed by the signs of ordinary language;

2nd. Absolute connexions or conditions among events capable of similar expression;

and of which the quæsitum or element sought is also the probability of some event or combination of events whose expression is known. Thus the elements of the question considered in my paper (On the Conditions by which the Solutions of Questions

in the Theory of Probabilities are limited, Philosophical Magazine, vol. viii. p. 91) are,

<p align="center">*Data.*</p>

Probabilities,   Prob. $x = c_1$ Prob. $y = c_2$ $\left.\begin{array}{l} \\ \\ \end{array}\right\}$ . . (1)
            Prob. $xz = c_1 p_1$ Prob. $yz = c_2 p_2$

Absolute connexion,

$$z(1-x)(1-y) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

<p align="center">*Quæsitum.*</p>

<p align="center">Prob. z.</p>

Here, beside the probabilities of the several events whose logical expressions are x, y, xz, and yz, we have given the absolute connexion $z(1-x)(1-y) = 0$, denoting (in the language of the Calculus of Logic) that the event z cannot happen in the absence of the events x and y. The quæsitum is the probability of the event whose expression is z. I design to investigate a general method of solving problems of this kind. Such a method, viewed through the range of its consequences, is entitled to be regarded as a general method in probabilities, because all solvable questions may be referred either directly, or through some intermediate principle, to the above class. And the hope which moves me to repeat here without substantial change the demonstration of such a method contained in my treatise on the Laws of Thought, is that of being able to set forth with greater fulness the distinctive principles upon which the demonstration depends, and of annexing to the final statement of the rule to which it leads, an important addition.

2. Probability I conceive to be not so much expectation, as a rational ground of expectation, and its numerical measure I define with mathematicians generally* as follows.

Definition. If, respecting any event, the mind is only able to form a number n of similar and mutually exclusive hypotheses, to none of which it is entitled to give any preference over any other, and if m of those hypotheses are favourable to the event, *i. e.* such that any one of them being realized, the event will happen, while the remaining hypotheses are unfavourable to it,

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* To quote, for example, Laplace's definition, "La théorie des hasards consiste à réduire tous les évènemens du même genre à un certain nombre de cas également possibles, c'est à dire, tels que nous soyons également indécis sur leur existence ; et à déterminer le nombre de cas favorables à l'évènement dont on cherche la probabilité. Le rapport de ce nombre à celui de tous les cas possibles est la mésure de cette probabilité, &c."—*Essai philosophique sur les Probabilités*, p. 7. Subsequently, Laplace speaks of the different "cases" as "hypotheses," which, indeed, they are.

i. e. favourable in the above sense to its *not* happening, the probability of the event is measured by the fraction $\frac{m}{n}$.

It does not, I think, need proof that the principles of the theory of probabilities must be derived either,—1st, from the nature of probability as set forth in its measure; or 2ndly, from its connexion with logic and language. Commencing with the former source, I remark that it is implied in the definition that probability is always relative to our actual state of information and varies with that state of information. Laplace illustrates this principle by supposing the following case. Let there be three urns, A, B, C, of which we are only informed that one contains black and the two others white balls; then, a ball being drawn from C, required the probability that the ball is black. As we are ignorant which of the urns contains black balls, so that we have no reason to suppose it to be the urn C rather than the urn A or the urn B, these three hypotheses will appear equally worthy of credit; but as the first of the three hypotheses alone is favourable to the drawing of a black ball from C, the probability of that event is $\frac{1}{3}$. Suppose, now, that in addition to the previous data it is known that the urn A contains only white balls, then our state of indecision has reference only to the urns B and C, and the probability that a ball drawn from C will be black is $\frac{1}{2}$. Lastly, if we are assured that both A and B contain white balls only, the probability that a black ball will issue from C rises into certitude. (*Essai Philosophique sur les Probabilités,* p. 9.) Here it is seen that our estimate of the probability of an event varies with our *knowledge* of the circumstances by which it is affected. In this sense it is that probability may be said to be relative to our actual state of information.

Let us, in further illustration of this principle, consider the following problem. The probability of an event x is measured by the fraction $\frac{a}{m}$, that of an event y by the fraction $\frac{b}{n}$, but of the connexion of the events x and y absolutely nothing is known. Required the probability of the event xy, *i. e.* of the conjunction of the events x and y.

There are (see definition) a cases in which x happens, to m cases in which it happens or fails; and concerning these cases the mind is in a state of perfect indecision. To no one of them is it entitled to give any preference over any other. There are, in like manner, b cases in which y happens, to n cases in which it happens or fails; and these cases are in the same sense equally

balanced.   Now the event xy can only happen through the combination of some one of the a cases in which x happens, with some one of the b cases in which y happens, while nothing prevents us from supposing any one of the m cases in which x happens or fails from combining with any one of the n cases in which y happens or fails.   There are thus ab cases in which the event xy happens, to mn cases which are either favourable or unfavourable to its occurrence.   Nor have we any reason to assign a preference to any one of those cases over any other. Wherefore the probability of the event xy is $\dfrac{ab}{mn}$.   Or if we represent the probability of the event x by p, that of the event y by q, the probability of the combination xy is pq.

It cannot be disputed that the above is a rigorous consequence of the definition adopted.   That new information might alter the value of Prob. xy is only in accordance with the principle (already exemplified from Laplace) of the *relative* character of probability.   It is only so far forth as they are *known*, that the connexions, causal or otherwise, of events can affect expectation. Let it be added, that the particular result to which we have been led is perfectly consistent with the well-known theorem, that if x and y are known to be *independent* events, the probability of the event xy is pq.   The difference between the two cases consists not in the numerical value of Prob. xy, but in this, that if we are sure that the events x and y are independent, then are we sure that there exists between them no hidden connexion, the knowledge of which would affect the value of Prob. xy; whereas if we are not sure of their independence, we are sensible that such connexions may exist.   Again, it is perfectly consistent with the known theorem, that if the probability of x is p, and the probability that if x happen y will happen is q, then the probability of the combination xy is pq.   For if we know nothing of the connexion of x and y, the occurrence of x will not affect our expectation of the occurrence of y, so that the probability that if x happen y will happen, will, in the actual state of our information, be the same as the simple probability of y, *i. e.* as q.

4. As from the simple data Prob. $x=p$, Prob. $y=q$ we deduce Prob. $xy=pq$, so from the same data we should have

Prob. $x(1-y)=p(1-q)$,   Prob. $(1-x)(1-y)=(1-p)(1-q)$&c.

And generally it may be shown, that if the probabilities of any events x, y, z are simply given, the probability of any combination of them expressed by F $(x, y, z . .)$ will be found by substituting in that expression for x, y, $z . .$ their given probabilities.

The general principle involved in the above deductions may be thus stated.

Principle I.—Probability is always relative to our actual state of information. Upon the actual connexions of events it depends no further than as such connexions are known to us.

This doctrine of the nature of probability, it may be added, has been fully recognized by acute and thoughtful minds approaching the subject from a point of view different from the mathematical one*.

I proceed to the statement of an important principle founded on the nature of language as an instrument of expression. It is, that in the theory of probabilities, as in every other branch of science, the solution of a question ought to depend upon the *information* conveyed in the data, and not upon the special elements or constructions of the language which may serve as the vehicle of that information. Now one very important point in which languages are observed to differ, is the selection of the objects or events to which simple terms are appropriated. In the rude infancy of nations, the number of such terms is small, and their application is confined within the limits of daily experience. With the progress of society the need of a wider vocabulary is felt, not merely for the expression of things unknown to former experience, but also for the purpose of abbreviation. Simple terms are invented, not solely for the representation of things wholly new, but for the more simple expression of things which it was before possible to express by a combination of terms. Whensoever in this gradual advance of language the combination of two simple terms is replaced by a new simple term, a definition or an equivalent series of ordinary propositions is introduced. Thus, if every combination of rain with snow becomes represented, for abbreviation, by the simple term "sleet," we virtually carry with us, whenever we use that term, the definition "sleet is rain with snow," or the equivalent train of propositions, "If there is sleet there is rain with snow," "If there is rain with snow there is sleet;" and that definition, or its equivalent propositions, we must, if need be, *express* as well as assume. Now it is manifest that there is no limit to this invention of simple terms, and consequent implication of propositions. In a language possessed of an infinite copiousness of diction, every object of experience, every combination of events, might thus be expressed by a simple term. Supposing that we had such a language at command, it is evident that we might in various ways express the data and the object of a question in the theory of probabilities. The events whose probabilities are given might, according to one mode of expression, appear as compound events

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* For instance, it is stated with great clearness in an extract from the commonplace book of Bishop Copleston, recently published by Archbishop Whately.

expressed by combinations of simple events; according to another mode, as simple events connected together by *definitions* or by implied *propositions*. Now the principle which I wish to assert is, that it is wholly indifferent which mode of expression we employ, provided that it be adequate to convey all the information we possess. Perhaps that principle may be more definitely stated as follows.

Principle II.—Any events which suffice simply, or by combination, for the expression of the data may be assumed as simple events and symbolized accordingly, provided that we explicitly determine the whole of the relations which implicitly connect them. To make plain my meaning, let it be supposed that observation has furnished the following elements of a problem :—

$$\text{Probability of rain} = p,$$
$$\text{Probability of rain with snow} = q;$$

the quæsitum of that problem being

$$\text{Probability of rain without snow.}$$

The expression of this problem by an observer in whose language there should exist no word for "snow," but in which every combination of rain with snow should be termed "sleet," would be as follows :—

1st. Probability of rain $=p$,
2nd. Probability of sleet $=q$, $\left.\right\}$ Data.
3rd. Sleet always implies rain.

Required probability of rain without sleet.

It is then affirmed that these two statements are equivalent. The expectation of a phænomenon cannot be affected by the mere mode of statement of it, and of the circumstances upon which it depends. As respects the two modes of statement in the above instance, it will be seen that in the former of them, one of the given probabilities is that of a compound event; in the latter, both the given probabilities are those of simple events between which an absolute relation (3rd) is affirmed to exist, and in terms of which the event whose probability is sought is directly expressed.

Now, beside that it is the most obvious course of procedure to determine *directly* the event whose probability is sought in terms of those whose probabilities are given, an object which we can always effect by the Calculus of Logic, there is a special reason why we should take this course. Consider the problem employed for the purpose of illustration in the first section of this paper. Representing the events xz and yz, since their probabilities are *given*, by s and t respectively, its data become

Prob. $x=c_1$, Prob. $y=c_2$, Prob. $s=c_1p_1$, Prob. $t=c_2p_2$; (3)
the elements x, y, s and t, here assumed (Principle II.) as

simple events, being connected by the relation

$$xyst + x(1-y)s(1-t) + (1-x)y(1-s)t + xy(1-s)(1-t)$$
$$+ x(1-y)(1-s)(1-t) + (1-x)y(1-s)(1-t)$$
$$+ (1-x)(1-y)(1-s)(1-t) = 1; \quad . \quad . \quad . \quad . \quad (4)$$

and the event whose probability w is *sought* being under the same conditions

$$xyst + x(1-y)s(1-t) + (1-x)y(1-s)t. \quad . \quad . \quad (5)$$

These results (4) and (5) are both given by the development (Laws of Thought, p. 322). The probabilities (3), together with the relation (4), are equivalent to the data of the problem as expressed in terms of x, y, and z in the section referred to. Now I remark that the mere probabilities (3) do not of themselves furnish any relations connecting x, y, s and t. The whole of the relation connecting those elements is given by (4), and it is given in the form of a logical equation, *i. e.* of an equation interpretable into a *proposition*. We possess of that relation an *explicit* and *available* knowledge. But it is not so with the relation connecting the elements x, y, z, when, as in the primary statement of the problem, these are assumed as simple events. We are explicitly informed that these elements are connected by the relation $z(1-x)(1-y) = 0$; but beside this, they are connected with each other in a complex manner through the data

Prob. $x = c_1$, Prob. $y = c_2$, Prob. $xz = c_1 p_1$, Prob. $yz = c_2 p_2$. These data exhibit both x and y as connected with z, and thereby also connected with each other. But that connexion is not of a kind which can be exhibited in an *explicit* form by means of propositions. And our consequent inability to express by any distinct and intelligible formula the implied relations among the elements x, y, z, renders it difficult to judge of the "reasonableness" or of the "anomalous" character of results in the expression of which these elements are employed*.

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* I need scarcely remark, that the statement of the problem furnished by (3), (4), and (5) will lead, and by the same method, to the conditions connecting c_1, c_2, $c_1 p_1$, $c_2 p_2$, and w, investigated in my former paper (vol. viii. p. 91). If we assume

Prob. $xyst = \lambda$	Prob. $x(1-y)s(1-t) = \mu$
Prob. $(1-x)y(1-s)t = \nu$	Prob. $xy(1-s)(1-t) = \rho$
Prob. $x(1-y)(1-s)(1-t) = \sigma$	Prob. $(1-x)y(1-s)(1-t) = \tau$

$$\text{Prob. } (1-x)(1-y)(1-s)(1-t) = \upsilon,$$

we shall have the following equations:

$$\lambda + \mu + \rho + \sigma = c_1$$
$$\lambda + \nu + \rho + \tau = c_2$$
$$\lambda + \mu \qquad = c_1 p_1$$
$$\lambda + \nu \qquad = c_2 p_2$$
$$\lambda + \mu + \nu \qquad = w$$
$$\lambda + \mu + \nu + \rho + \sigma + \tau + \upsilon = 1,$$

whence the conditions in question may be deduced.

The above, together with the general principles of symbolical algebra, suffice for the ground of the following demonstration, which differs from that contained in the Laws of Thought only in applying throughout the familiar illustration of the urn.

Demonstration.

Let those events, which in the actual language of the problem appear as simple events, be represented by the logical symbols $x, y, z \ldots$ Any event whose probability is given or sought may then be represented upon the principles of the Calculus of Logic by a *function* of those symbols. Thus the event which consists in the concurrence of x and y jointly with the absence of z will be represented by $xy(1-z)$; and the event which consists in the happening of some one alone of the events x, y, z will be represented by the function

$$x(1-y)(1-z) + y(1-x)(1-z) + z(1-x)(1-y);$$

of which function, it is to be observed, that the several terms connected by the sign $+$ are called constituents. If we express generally functions of the above description by the ordinary functional symbols $\phi, \psi, \theta, \mathrm{F}$, &c., we may thus express the problem which we have to consider in the following manner.

Probabilities given :—

$$\text{Prob. } \phi(x, y, z) = p, \text{ Prob. } \psi(x, y, z) = q, \text{ \&c. } \quad . \quad . \quad (1)$$

Annexed absolute conditions :—

$$\theta(x, y, z \ldots) = 0, \text{ \&c. } \quad . \quad . \quad . \quad . \quad (2)$$

Quæsitum, or probability sought :—

$$\text{Prob. } \mathrm{F}(x, y, z \ldots). \quad . \quad . \quad . \quad . \quad (3)$$

Now the most obvious mode of procedure is to seek to express the event whose probability is sought, explicitly in terms of the events whose probabilities are given. To do this, we must, in accordance with Principle II., regard all these as simple events, expressing them by new logical symbols w, s, t, &c. Let then

$$\phi(x, y, z \ldots) = s, \quad \psi(x, y, z \ldots) = t, \quad \mathrm{F}(x, y, z \ldots) = w. \quad (4)$$

From the logical equations (2) and (4) we can now determine w in terms of s, t, &c. The solution will be of the form

$$w = \mathrm{A} + 0\mathrm{B} + \frac{0}{0}\mathrm{C} + \frac{1}{0}\mathrm{D}. \quad . \quad . \quad . \quad (5)$$

Here A, B, C, D are functions of s, t, &c., and the several terms of the development are, by means of their coefficients, thus interpretable.

1st. A represents those combinations of the events s, t, &c. which must happen if w happen.

2nd. B those combinations which cannot happen if w happen, but may otherwise happen.

3rd. C those combinations which may or may not happen if w happen.

4th. D those combinations which cannot happen at all.

And the above representing all possible combinations, we have

$$A + B + C + D = 1. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Now there are many problems in which the combination denoted by C does not present itself. Such is the one considered by Mr. Cayley and myself, and commented on by Mr. Wilbraham. As the principle of solution is the same in this as in the more general class of problems in which C does appear, I shall, for simplicity, confine myself to the simpler case. The event w, then, consists solely of that combination of the simple events s, t, &c. which is denoted by A, and the sole condition to which those events are subject is

$$D = 0, \text{ or } A + B = 1; \quad . \quad . \quad . \quad . \quad (7)$$

these logical equations being, by virtue of the *necessary* equation (6), strictly equivalent when C does not make its appearance in the development.

The problem may now be briefly stated as follows. The events s, t, &c. are subject to the condition (7), and at the same time their respective probabilities are

Prob. $s = p$, Prob. $t = q$, &c.

Required the value of Prob. A.

Now let us consider whether, upon the familiar notion of an urn containing balls, we can construct a problem whose expressed data shall be in all respects the same as the above, and which shall at the same time admit of definite solution.

And, in the first place, it is manifest that any event or combination of events may be represented by the issuing of a ball possessing a particular quality, or combination of qualities from an urn. Thus the event s may be represented by the issuing of a ball possessing a particular quality which we will term the s-quality, the event t by that of a ball possessing the t-quality, and so on. In like manner the event st, or the combination of the events s and t, may be represented by the issuing of a ball possessing at once the qualities s and t. And generally the events A, B, D, whatever combinations of the symbols s, t .. these letters may stand for, may be represented by the issuing of balls possessing the corresponding qualities or combinations of qualities.

And as every species of events can thus be represented by the issuing of a ball of a particular species from an urn, so every

problem relating to events may be represented by a corresponding problem relating to the issuing of balls from an urn. If, in such imagined problem, any events *s*, *t*, &c. so enter as that nothing is known or can be inferred respecting their connexion, they must be treated (Principle I.) as if they were *independent*, and therefore the balls by whose issue they are represented must be regarded as free from any *nexus* affecting their issue. On the other hand, if the events *s*, *t*, &c. are subject to any condition as D=0, such condition must be introduced by the supposition of a *nexus* simply forbidding the issue of balls of the species D, without affecting the freedom of the other balls. Such a nexus we may suppose to be established by the attachment of every ball of the species D by a thread to the walls of the urn. All possible issues are thus restricted to balls of the species A or B, so that the condition D=0 is equivalent, as we have before seen, to the condition A+B=1.

The general problem may therefore be represented as follows:—

An urn contains balls whose species are expressed by means of the qualities *s*, *t*, &c. and their opposites, concerning the connexion of which qualities nothing is known. Suddenly all balls of the species D are attached by threads to the walls of the urn, and this being done, there is a probability *p* that any ball drawn is of the species *s*, a probability *q* that it is of the species *t*, and so on. What is the probability that it is of the species A, supposing that A and D denote mutually exclusive species of balls, each defined by means of the properties *s*, *t* and their opposites?

Let us, for simplicity, represent A+B by V, and let us represent by V_s the aggregate of constituents in V of which *s* is a factor, by V_t the aggregate of constituents of which *t* is a factor, and so on. Then, according to the principles of the Calculus of Logic, we shall have the following interpretations, viz.—

V = that event which consists in the drawing of a ball which is not of the species D.

V_s = that event which consists in the drawing of a ball which is of the species *s* and is not of the species D.

V_t = that event which consists in the drawing of a ball which is of the species *t* and is not of the species D.

Now let the total number of balls in the urn be N, and let S be the number which are of the species *s*, T the number which are of the species *t*, &c. Hitherto *s* and *t* have been used only as *logical* symbols expressing *events*. Let us now introduce a new set of symbols *s*, *t*, &c., to be used in a *quantitative* acceptation, to denote the numerical ratios $\frac{S}{N}$, $\frac{T}{N}$, &c. Then we have

s (quantitative) = probability, before the nexus, of the event s,
t (quantitative) = probability, before the nexus, of the event t,

and so on. And hence V, V_s, V_t, &c. *quantitative*, representing what the same expressions *logical* become when we change, as above, the signification of s, t, &c., we have the following derived probabilities (Principle I.).

Probabilities before the nexus :—

V (quantitative) = probability of the drawing of a ball not of the species D.

V_s(quantitative) = probability of the drawing of a ball of the species s but not of the species D.

V_t(quantitative) = probability of the drawing of a ball of the species t but not of the species D.

A (quantitative) = probability of the drawing of a ball of the species A.

Now, after the *nexus*, the probability of the drawing of a ball of the species s is obviously the same as the probability before the nexus, that if a ball not of the species D be drawn, it will be of the species s. Hence

$$p = \frac{\text{Prob. (before nexus) of } s \text{ not D}}{\text{Prob. (before nexus) of not D}}$$

$$= \frac{V_s}{V}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

And thus we form the series of *quantitative* equations,

$$\frac{V_s}{V} = p, \quad \frac{V_t}{V} = q, \text{ &c.} \quad . \quad . \quad . \quad . \quad (9)$$

Again, the probability after the nexus, of the event A, is equal to the probability before the nexus, that if a ball not of the species D be drawn it will be of the species A,

$$= \frac{\text{Prob. (before nexus) of A not D}}{\text{Prob. (before nexus) of not D}}$$

$$= \frac{\text{Prob. (before nexus) of A}}{\text{Prob. (before nexus) of not D}}.$$

Since the events A and D are mutually exclusive,

$$= \frac{A}{V}.$$

Hence representing Prob. w by u, we have

$$u = \frac{A}{V}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The solution of the problem is now completed. The values of

the ratios *s*, *t*, &c. being found from (9), must be substituted in (10). These ratios being positive fractions, we must employ a set of values of *s*, *t*, &c., which consists solely of positive fractions. It will hereafter be shown, that when the problem is a *real* one, the system (9) furnishes one, and only one set of values answering the required description : that set must therefore be taken. This is the only addition required to the general rule as given in the Laws of Thought.

The combined systems (9) and (10) may be elegantly deduced by the following method, originally communicated to me by Professor Donkin.

The probabilities, before the nexus, of the events V, V_s, V_t.. and A are the corresponding *quantitative* functions V, V_s, V_t.. and A. The probabilities of the same events after the nexus are 1, p, q.. and u respectively. Now the only effect of the nexus is to exclude a number of hypotheses unfavourable to the happening of the above events, without affecting the cases favourable to their happening. Hence the several probabilities have to each other the same ratio before the nexus as after, and therefore

$$V : V_s : V_t .. : A = 1 : p : q .. : u;$$

or

$$\frac{V_s}{p} = \frac{V_t}{q} .. = \frac{A}{u} = V,$$

a system equivalent to the system (9) and (10).

The investigation is conducted in the same manner when the function C presents itself in the final logical development (5), and the general rule thus established is the following :—

Rule.—Form the symbolical expressions of the events whose probabilities are given or sought, and equate such of them as relate to compound events to a new set of logical symbols, *s*, *t*, &c. Express also any absolute conditions which may be given in the data. From the combined system determine by the Calculus of Logic, *w*, the event whose probability is sought in terms of all the events *s*, *t*, &c. whose probabilities are given, and let the result be

$$w = A + 0B + \frac{0}{0}C + \frac{1}{0}D.$$

Then representing the aggregate $A + B + C$ by V, and the sum of those constituents in V of which *s* is a factor by V_s, and so on, form the algebraic system of equations

$$\frac{V_s}{p} = \frac{V_t}{q} .. = V, \quad . \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

$$\text{Prob. } w = \frac{A + cC}{V}, \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

wherein *p*, *q*, &c. are the given probabilities of *s*, *t*, &c.

If the problem be a real one, the system (I.) *will furnish one set, and only one set, of positive fractional values of* s, t, &c., *which, substituted in* (II.), *will determine Prob. w.*

The interpretation of *c*, when it appears in the solution, is

$$\frac{\text{Prob. } Cw}{\text{Prob. } C},$$

and it indicates the new experience requisite to complete the solution of the problem.

If the system (I.) *does not furnish a single system of positive fractional values of* s, t, &c., *the problem is not a real one, and does not in its statement represent a possible experience.*

The passages in italics contain the additions to the rule as it is presented in the Laws of Thought.

In concluding this paper, I shall briefly consider the only two objections which have at any time occurred to my own mind as likely to occasion a difficulty in the reception of the above results.

1st. It may be, and indeed it has been, urged that the logical calculus upon which the investigation proceeds does not constitute a science or represent "reality," being only based upon a system of "substituted ratios."

To this it is replied, that pure science, as such, is concerned only with ratios or relations. To know things as they are in themselves, is the professed but unattainable object of a so-called philosophy proper. It is, however, here maintained that the logical calculus does represent reality and constitute science, inasmuch,—1st, as the laws of thought upon which it is founded, and which it expresses by the fundamental equations $xy = yx$, $x^2 = x$, &c. are not fictitious, but are derived from a real analysis of the intellectual operations; 2nd, as it is a fact, and not an assumption, that the laws thus determined are formally identical with the laws of a certain properly defined species of arithmetic; 3rd, as it accords with the catholic objects of science to avail itself of all discovered laws and relations, without regard to the fashion of the schools or the prescription of ancient usage.

2nd. It may be objected, that, although in the representative problem of the urn we can readily pass in thought from a system of balls having an actual physical nexus to the same system free from that nexus, we cannot, in the represented problem in which the events *s, t,* &c. are subject to the logical and therefore necessary connexion $D = 0$, interpret to ourselves the same events as freed from that connexion; and therefore the problem of the urn does not completely and adequately represent the problem for which it is substituted, inasmuch as in the one case the nexus or condition implied by the equation $D = 0$ is merely actual, while in the other case it is not only actual but necessary.

It is replied, that this necessity may be regarded as merely posterior to some act of limitation by which the events *s, t,* &c., previously of larger comprehension, became restricted to that particular interpretation in terms of *x, y,* &c. which they bear in the problem, and which is the foundation of the logical necessity referred to. What that larger comprehension is, it is wholly unnecessary to attempt to define. It suffices, upon the general grounds of symbolical algebra, to apply to the inverse process of the removal of a nexus, the formal laws which are derived from the direct and always interpretable case of its imposition. I regard this as a principle, which, though capable of verification in innumerable instances, does not rest simply upon the cumulative evidence afforded by such instances, but has a real foundation in the intellectual constitution.

Finally, as respects the mode in which the aforesaid logical necessity has been represented in the example of the urn, it may be remarked that it involves no more than is implied in the various figures by which, in different languages, the idea of necessity has been symbolized. For in each of those figures we have presented to us the notion of something which has once been free, but has ceased to be so through a material act, or a positive determination*. And any one of these modes of illustration might with equal propriety have been adopted.

The verification of these results will be considered in my next paper.

Lincoln, Sept. 30, 1854.

LV. *On some simultaneous Observations of Rain-fall at different points on the same Mountain-Range.* By W. J. Macquorn Rankine, *Civil Engineer, F.R.SS. Lond. and Edinb. &c.*†

1. THE question of the relative proportions of rain which fall at points having different situations, levels, and aspects on the same mountain-range, is one which, besides its scientific interest, is of great practical importance, especially with reference to the water-supply of large towns.

2. The observations here recorded are intended as a small contribution to our knowledge of this subject. They were made

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* Witness the supposed derivation of the Latin *necesse* from *nexus,* of the Greek εἱμαρμένη, from a verb signifying division by the casting of lots, of the word *fate,* &c. The higher limitation implied by the addition of such terms as *absolute* (*e. g.* absolute necessity) is curiously derived, not from the direct idea of physical restraint, but from the converse one of the removal of all restraint upon the restraining power.

† Communicated by the Author; having been read to the British Association for the Advancement of Science, Section A, at Liverpool, Sept. 1854.

daily and simultaneously for a period of 151 consecutive days, from the 13th of July to the 12th of December 1845, at two points on the north-western slope of the Pentland Hills near Edinburgh. The long lapse of time previous to their publication has been occasioned by the fact, that the fair copy of the observations, with various calculations founded on them, accidentally passed out of my possession not long after it had been prepared; and it was only a short time since that I happened to find the original registers, which I have reduced anew.

3. The range of the Pentland Hills extends in a north-east and south-west direction for about thirteen miles. Its north-east extremity is about five miles from Edinburgh towards the south, and seven miles from the Frith of Forth. For about four miles in the middle of its course the range is double, consisting of two parallel chains of summits separated by a deep and narrow valley called Glencorse, from which a considerable portion of the water-supply of Edinburgh is derived. This valley is traversed by a stream which runs towards the north-east, then turns at right angles and escapes towards the south-east through a gorge, now occupied by the embankment of one of the reservoirs of the Edinburgh Water Company.

4. The summits of the Pentland Hills vary from 1400 to 1900 feet in height above the sea; the ridges between them, from 1000 to 1300 feet. The highest summits are in the double chain, near the middle of the range.

5. The south-west extremity of the Pentland Hills is a point of divergence of tributary streams of the Forth, Clyde and Tweed. To the west and south-west of this point rise some small tributaries of the Clyde. From the south-eastern face of the hills, near the point of divergence, flow some tributaries of the Tweed; further towards the north-east and from the central valley, tributaries of the Frith of Forth. On the north-western face of the range are the sources of the water of Leith and some smaller tributaries of the Frith of Forth; and on the same face, near its south-western end, some sources of the Almond, also a tributary of the Frith of Forth. From the south-western extremity of the Pentland range already referred to, a range of low hills of from 1000 to 800 feet in height extends towards the west-north-west, being part of the boundary between the basin of the Forth and that of the Clyde.

6. The surface of the Pentland Hills is covered chiefly with pasture and heath, with a few patches of peat. Ample details of the topography and geology of this range may be found in Mr. Charles Maclaren's work on the Geology of Fife and the Lothians.

7. The two points at which the observations of rain-fall now

referred to were made, were situated on the north-western face of the Pentland range near its south-western extremity, amongst the sources of the river Almond. Both the gauges were placed as near as possible to the surface of the ground, in open and regularly-sloping pasture, far from any sheltering objects.

The lower gauge, designated by H, was about 700 feet above the level of the sea, three miles and a quarter to the north-west of the line of summits of the Pentland Hills, about four miles to the north-east of the range of low hills before mentioned, and ten miles southward from the Frith of Forth.

The higher gauge, designated by G, was about 900 feet above the level of the sea, or 200 feet above the gauge H, one mile and a half to the north-west of the line of summits of the Pentland Hills, one mile and three-quarters to the north-east of the range of low hills, and two miles and a half from the gauge H in a direction south-by-west.

8. The rain-fall was recorded by a different observer for each gauge every morning about nine o'clock. Although minute fractions were not attended to in the readings, so that comparisons between the quantities of rain at the respective gauges on *single days* are of little value, yet I am satisfied of the accuracy of the result of any comparison embracing a period of several days.

The vicinity of situation and similarity of aspect of those two gauges naturally caused them to receive the rain-fall of the same masses of cloud, so that very few instances occurred of rain falling at one gauge without a fall also taking place at the other within the space of a few hours, if not simultaneously.

9. In the first of the annexed tables, therefore, the depths of rain-fall at the gauges G and H are compared for each *period of rainy weather*. There are seventeen of these periods in the table, varying in length from two to eleven days, and separated by periods of drought. The first period, however, from the 13th to the 20th of July, must be taken with the qualification, that the quantities of rain set down for it are merely the sums of a number of brief showers occurring at intervals. At the foot of the table the total depths of rain are compared for the whole period of observation, 151 days.

The mean ratio set down at the foot of Table I. in the fourth column, shows that the total rain-fall at the gauge G exceeded that at the gauge H in the ratio of one to one and a quarter.

The ratios for the seventeen periods of rain deviate from this mean amount, upwards and downwards, in a somewhat irregular manner, but evince, on the whole, a tendency to increase with the approach of winter.

10. In the second table, the depths of rain-fall at the gauges H and G are compared for each of the calendar months, August,

September, October, and November 1845, and for the period consisting of these four months. The ratios for the several months in the fourth column of this table show a much less extent of deviation from the mean ratio of one to one and a quarter than those for shorter periods; and in them also is evident a tendency to increase on the approach of winter.

11. It is a question of some interest whether the excess of the rain-fall at the gauge G above that at the gauge H is principally caused by the greater elevation of the ground at the former gauge, or by the greater proximity of the gauge G to the chain of summits of the mountain range.

In order to throw some light on this question, there are added in the fifth column of Table II. the depths of rain, as published by Mr. Beardmore during the four months referred to, at a gauge which I have designated by F, 734 feet above the sea, situated near the embankment of the Edinburgh Water Company in the gorge of the Valley of Glencorse, and in the immediate vicinity of some of the highest summits of the range. In the sixth column are given the ratios of the depths of rain at the gauge F to those at the gauge H for the four months separately, and for the whole period.

From these data it appears, that although the gauge F was at nearly the same elevation with the gauge H, it received during the four months in question a depth of rain somewhat greater than that at the gauge G, nearly 200 feet higher. This was most probably the effect of the greater proximity of the gauge F to the highest summits of the range.

The ratios of the depth of rain at F to that at H for the different months vary in a very irregular manner; probably because those points are separated by the mountain-range, and receive the rain-fall of different masses of cloud and different winds.

12. In the third table the total depths of rain-fall during the four months at each of the gauges H, G, F, are compared with the depth registered in a gauge denoted by E during the same period at the residence of Mr. Adie, immediately to the south of Edinburgh, and a little more than 200 feet above the sea. The rain-fall at this last point is very much less than that at any one of the points on the mountain-range.

13. The rain-fall at points still nearer the summits of the hills than the gauges G and F must have been considerably greater than that indicated by these gauges. This appears from the facts, that the total flow of water during the period of observation, of a tributary stream of the river Almond, gauged by me at a point near G, corresponded to an effective depth of rain-fall only *one-thirteenth* part less than that recorded by the rain-gauge; and that the flow from the valley of Glencorse, as gauged by

Mr. Beardmore in 1846 and 1847, corresponded to an effective depth of rain-fall *somewhat exceeding* that at the rain-gauge F.

14. I do not bring forward the limited number of observations and comparisons here recorded as possessing any great value in themselves, but as capable of contributing to the attainment of useful conclusions when combined with other data of the same kind.

Table I.—Comparison of Rain-fall by periods of Rain.

Dates (days commencing at 9 A.M.).	Gauge H, inches.	Gauge G, inches.	Ratio $\frac{G}{H}$.
1845, July 13 to 30.	0·5	0·3	0·60
... 31 to Aug. 5.	0·9	1·0	1·11
Aug. 7 to 9.	0·3	0·4	1·33
... 10 to 13.	1·1	1·3	1·18
... 18 to 22.	0·9	1·2	1·33
... 23 to 27.	0·6	0·7	1·17
Sept. 14 to 17.	0·2	0·3	1·50
... 18 to 20.	1·5	1·7	1·13
... 21 to 23.	1·0	0·9	0·90
... 25 to Oct. 6.	4·4	4·1	0·93
Oct. 7 to 14.	0·6	0·8	1·33
... 15 to 22.	1·2	2·3	1·92
... 27 to 31.	1·0	1·8	1·80
Nov. 6 to 12.	0·4	0·4	1·00
... 17 to 21.	1·1	1·5	1·36
... 25 to Dec. 4.	1·7	2·8	1·65
Dec. 5 to 12.	0·4	0·8	2·00
Total for 151 days......	17·8	22·3	1·25 mean ratio.

Table II.—Comparison of Rain-fall by Calendar Months.

Months.	Gauge H, inches.	Gauge G, inches.	Ratio $\frac{G}{H}$.	Gauge F, inches.	Ratio $\frac{F}{H}$.
August 1845	3·5	4·2	1·20	5·38	1·54
September	4·3	5·0	1·15	4·83	1·12
October ... ...\...........	5·6	6·9	1·23	8·01	1·43
November	2·9	4·3	1·48	2·90	1·00
Total rain and mean ratios...	16·3	20·4	1·25	21·12	1·30

Table III.

	Gauge H, inches.	Gauge E, inches.	Ratio $\frac{E}{H}$.	Inverse ratio $\frac{H}{E}$.	Ratio $\frac{G}{E}$.	Ratio $\frac{F}{E}$.
Aug., Sept., Oct. and Nov. ...	16·3	13·09	0·80	1·26	1·56	1·62

LVI. *Observations on Meteorolites or Aërolites, considered Geographically, Statistically, and Cosmically, accompanied by a complete Catalogue.* By R. P. GREG.

THERE is no occasion, in continuation, to enter into details concerning the phænomena attending the fall of meteorites and fire-balls, &c., or give a list of the analyses which have from time to time been made of various meteoric irons and stones. Suffice it to say, that no new chemical element has yet been discovered in these bodies, though several new mineral compounds have been observed. Most, indeed nearly all, the simple chemical elements have been detected in them.

Some consist of pure iron; others of iron alloyed with nickel, perhaps also accompanied with small quantities of carbon, chromium, cobalt, arsenic and phosphorus; and some few are mechanically combined with crystallized olivine; the *majority* have, however, a common or normal character, both internally and externally. They have been variously classified; as by Prof. Shepard in the following way, treating them as it were mineralogically:—

<div align="center">Class I.—METALLIC.</div>

Order 1.

Malleable, homogeneous
{ Sec. I. Pure.
Sec. II. Alloyed { Closely crystalline.
Coarsely crystalline.

Order 2.

Malleable, heterogeneous
{ Amygdalo-peridotic.
Amygdalo-pyritic.
Pyrito-plumbaginous.

Order 3.

Brittle : . . . . . { Pure.
Alloyed.

<div align="center">Class II.—STONY.</div>

Order 1.

Trachytic. . . .
{ Sec. I. Peridotic { Coarse-grained.
Fine-grained.
Sec. II. Pyroxenic.
Sec. III. Chladnitic.
Sec. IV. Carbonaceous.

Order 2.

Trappean. . . . { Sec. I. Homogeneous.
Sec. II. Porphyritic.

Order 3.   Pumice-like.

Probably, and partly in connexion with this similarity with the chemical elements and even minerals of our own planet, has been developed the theory of the *non-extra*-terrestrial origin of

meteoric irons and stones ; a theory principally supported by the chemists and electricians, as Sir H. Davy, Fusinieri, M. Biot, Prof. Shepard, M. F. G. Fischer and others. Before concluding this paper I shall shortly allude to this theory, as it bears strongly on the general subject. I cannot do better than again quote from Prof. Shepard. (See his Report on American Meteorites, published in the American Journal of Science.)

"The extra-terrestrial origin of meteoric stones and iron masses seems likely to be more and more called in question, with the advance of knowledge respecting such substances, and as additions continue to be made to the connected sciences; I may therefore take an early occasion of presenting some views, founded partly upon Biot's theory of the aurora borealis, which seem to favour such an origin of meteorites.

"The recent study of those frequently occurring and widespread atmospheric accumulations of meteoric dust (a single case being recorded where the area must have been thousands of square miles in extent, and where the quantity of earthy matter precipitated must have been from 50 to 500,000 tons in weight), makes known to us the vast scale on which terrestrial matter is often pervading the regions of the upper atmosphere, and prepares us to appreciate the mode in which peculiar constituents of meteorites may be translated to those remote distances, where, according to the theory of Biot, the clouds of meteoric dust are retained.

"Great electrical excitation is known to accompany volcanic eruptions, which may reasonably be supposed to occasion some chemical changes in the volcanic ashes ejected; these being wafted by the ascensional force of the eruption into the regions of the magneto-polar influence, may there undergo a species of magnetic analysis. The most highly magnetic elements (iron, nickel, cobalt, chromium, &c.), or compounds in which these predominate, would thereby be separated and become suspended in the form of metallic dust, forming those columnar clouds so often illuminated in auroral displays, and whose position conforms to the direction of the dipping-needle. While certain of the diamagnetic elements (or combinations of them), on the other hand, may under the control of the same force be collected into different masses, taking up a position at right angles to the former (which Faraday has shown to be the fact in respect to such bodies), and thus produce those more or less regular arches, transverse to the magnetic meridian, that are often recognized in the phænomena of the aurora borealis.

"Any great disturbance of the forces maintaining these clouds of meteor-dust, like that produced by a magnetic storm, might lead to the precipitation of portions of the matter thus suspended.

If the disturbance was confined to the magnetic dust, iron-masses would fall; if to the diamagnetic dust, a non-ferruginous stone; if it should extend to both classes simultaneously, a blending of the two characters would ensue in the precipitate, and a rain of ordinary meteoric stones would take place.

" As favouring this view, we are struck with the rounded, hailstone-like form of many of the particles of composition (even though consisting of widely different substances) in nearly all stones, and even in many of the iron masses. Nor are these shapes to be referred to fusion; they evidently depend upon a cause analogous to that which determines the same configuration in hailstones themselves.

" The occasional raining of meteorites might therefore on such a theory be as much expected as the ordinary deposition of moisture from the atmosphere. The former would originate in a mechanical elevation of volcanic ashes and in matter swept into the air by tornadoes, the latter from simple evaporation. In the one case, the matter is upheld by magneto-electric force; in the other, by the law of diffusion which regulates the blending of vapours and gases, and by temperature. A precipitation of metallic and earthy matter would happen on any reduction of the magnetic tension; one of rain, hail or snow, on a fall of temperature. The materials of both originate in our earth. In the one instance they are elevated but to a short distance from its surface, while in the other they appear to penetrate beyond its furthest limits, and possibly to enter the interplanetary space; in both cases, however, they are destined, through the operation of invariable laws, to return to their original repository."

That large falls of dust and other substances do occasionally take place is without doubt, as well as that volcanic dust is sometimes shot up into the air and carried great distances; but that it is this *same* matter which is afterwards sustained or solidified by magnetic action, there is no proper evidence to prove. The fall of a meteorite is usually preceded by an explosion, and a *scattering*, rather than *uniting*, of fragments or bodies; evidence rather of some larger part, or whole, entering our atmosphere from without, and bursting or cracking from sudden heating, into larger or smaller particles according to the original nature and texture of the body itself. Humboldt, in his ' Cosmos,' decidedly expresses the opinion that the nature of these meteoric stones, and the phænomena accompanying their fall, are such as to preclude the idea of their having been condensed from minute matter or from a gaseous state, in a short interval of time: he also states that meteoric masses kindle and become luminous at elevations which must be supposed to be almost entirely deprived

of air, and frequently explode at great elevations. Their enormous and probably planetary velocity, their oblique, nay, sometimes horizontal direction, frequently in a retrograde or opposite direction to the earth's motion, are all perfectly subversive of the idea of these meteoric masses having a terrestrial or atmospheric origin. And there are other objections to M. Biot's and Prof. Shepard's theory, such as the question, whence comes the large quantity of nickel in meteoric irons? It is an extremely rare metal on the earth, and is only found in a few localities; nor does it, that I am aware of, constitute any portion of volcanic matter hitherto analysed.

It is not to be denied that there exist some phænomena of the meteoric class which have an atmospheric and therefore terrestrial origin; there are, we know, cases of electric action producing certain kinds of fireballs; there are falls of dust, black rain, and even of viscid substances; but it is necessary to separate these cases from the regular meteoric masses, stone or iron, and not indiscriminately attempt to account for all these things by one theory, however ingenious, or howsoever in particular cases and to a certain extent, correct. La Grange, Arago and Humboldt all agree in rejecting the atmospheric origin of aërolites.

Having thus examined and rejected the theory of a *terrestrial* origin of meteoric masses, I shall conclude by summing up the principal points I have endeavoured to establish.

First. That the deposition of meteoric matter on the surface of the earth has *not* been, all things considered, otherwise than uniform, *i. e.* there is no decided tendency to *local* deposition.

Secondly. That their origin is not within the limits of the earth's atmosphere.

Thirdly. That they are probably distinct from ordinary luminous meteors, as regards both their physical nature and orbits, and may also exhibit periodicity. (See Table F.)

Fourthly. That their period of least common occurrence takes place when the earth is on the side of the winter solstice in *perihelion*; while, on the other hand, the period of most frequent occurrence is when the earth is in *aphelion,* and the mean system or mass of the asteroids in their *perihelion.*

Fifthly. That they may reasonably be considered as belonging to the group of planetoids or asteroids, and to partake, therefore, of the proper nature and conditions of planetary bodies.

———

Note I. p. 332.—One circumstance may be mentioned as being rather singular, which is, the extraordinary number of meteoric irons, discovered within a comparatively short period in the United States,

viz. thirty-four; while only *one* has been found in France, and but *one* in Great Britain.

In Mexico eight meteoric irons have been discovered and described, but there is no recorded or historic instance of a stone fall; yet in the United States there have been seventeen falls of stones this century, and one observed iron fall.

There is no accounting for these apparent irregularities; possibly several of the Mexican and United States iron meteoric masses have been the result or produce of one shower or explosion.

The proportion of stone to iron falls may be taken at 25 to 1, *i. e.* 96 per cent. of all that fall consist of stony matter; so that for the thirty-four iron masses found in the United States there may have been $34 \times 96 = 3264$ stone falls.

Note II. p. 334.—It is remarkable, that while December has only nine falls recorded, *five* out of these should have occurred on the 13th of the month, and *one* on the 14th. Five fell within the space of only twelve years, and *two* fell on the 13th December 1803, at two distinct localities.

In looking through Prof. Powell's 'Catalogues of Luminous Meteors,' and various journals, there are described only two meteors and two small falling stars for any 13th day of December.

Note III. p. 336.—Professor Cappocci of Naples, in a letter to M. Arago (given in the *Comptes Rendus* for August 1840), endeavours, though I think not very successfully, to establish, not only a coincidence in the fall of aërolites and luminous meteors for the 16th and 17th of July, but assigns to them a regular recurring period of five years, and concludes by supposing that they are " the result of an aggregation of cosmical atoms dispersed in space; atoms which are constrained to unite themselves by contrary poles in consequence of magnetic attraction." And he seems to consider comets, aurora boreales, meteors and aërolites as various resultants from bands or currents of nebulous matter, existing in planetary space, in a state of magnetism more or less intense.

Note IV. p. 336.—It can hardly be imagined that the small fragments and atoms which usually constitute aërolites can have any luminosity, whether reflected or inherent. It is possible, however, they may form the more solid part or nucleus of larger and less solidified bodies. That ordinary falling stars, and more particularly the luminous meteors observable in the great periodic displays of August and November, are self-luminous can hardly admit of doubt. It may be mentioned that Pallas has probably irregular and angular surfaces, like the majority of meteoric stones, and that Ceres is apparently surrounded with a very dense atmosphere; a circumstance perhaps also sometimes the case, on a far smaller scale, with meteorolithic fire-balls. Respecting the cause of the supposed breaking up of a planet between Mars and Jupiter, Mr. Nasmyth, at a recent

meeting of the British Association, ingeniously suggested that its disruption might have occurred when the planet had arrived at some such condition or state of tension (whilst cooling) as that known to exist in a Prince Rupert drop, which, as is well known, shivers to pieces on the slightest injury to the surface.

Table F.—Showing the days of each month on which aërolites have fallen to the earth.

Months.																		
January	4	9	9	10	12	13	15	16	28									
February	3	10	10	13	15	15	15	18	18	18	19	19	25	27	27			
March	1	2	3	6	8	12	12	13	14	15	19	21	22	22	25	25	30	30
April	1	5	5	11	12	13	15	17	18	19	26	28	29					
May	9	9	10	17	17	17	18	19	19	20	22	26	26		28			
June	2	4	5	6	9	9	12	12	13	15	16	20	20	21	22			
July	3	3	4	4	7	8	12	14	17	18	22	24	24	26	26	26	30	
August	4	4	5	6	7	9	10	15	15	18	20							
September	3	4	4	5	5	9	9	10	10	13	13	14	16	19				
October	1	1	2	3	5	6	8	12	13	13	14	20	25	27	31			
November	5	7	11	13	17	20	23	25	27	29	29	29	29	30	30			
December	11	13	13	13	13	13	14	25	28									

1803

Note *a*.—Epochs supposed to be periodical in displays of "luminous meteors," are here inserted for the purpose of comparing the results with Table F.:—

April	22–25.
July	17–19.
August	9–13.
October	16–18.
November	10–14.
Ditto	27–29.
December	8–12. (?)

Note *b*.—Epochs when it would appear that the falls of aërolites may be periodical:—

February	15–19.
March	19–25.
May	17–20.
June	9–16. (?)
Ditto	20–22
July	22–26.
September	9–14.
November	29–30.
December	11–14.

Table G.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
Period of 63 years	1825	1853	1849	1851	1848	1843	1847	1841	1854	1849	1850	1846
	1824	1848	1843	1844	1846	1841	1843	1835	1852	1844	1846	1836
	1810	1847	1841	1842	?1831	1838	1842	1829	1843	1838	1839	1833
	1796	1839	1820	1838	1827	1834	1840	1823	1833	1827	1835	1813
		1830	1818	1812	1808	1828	1837	1822	1829	1824	1833	1807
		1827	1813	1812	1806	1822	1835	1818	1826	1819	1822	1803
		1825	1811	1808	1791	1821	?1831	1812	1825	1815	1820	1803
		1824	1807	1804		1819	1820	1810	1822	1803	1814	1798
		1818	1806	1799		1818	1811	?1800	1822	1791	1811	1795
		1815	1805	1795		1809	1810		1814		1805	
		1814	1798			1805	1803		1813			
		1796	1796			1794	1790		1808			
									1802			
Period of 100 years	1717	1785	1683	1780	1751	1752	1766	1789	1775	1787	1773	
	1697	1671	1654	1750	1698	1723	1755	1766	1768	1750	1768	
	1683	1647	1636	1620	1680	1668	1753	1738	1753	1740	1639	
	1622			1715	1677	1635	1727	1650	1650	1674	1627	
				1620			1725	1642				
							1647	1828				
							1635	1618				
Period of 200 years	1585		1596	1540	1580	1591	1581		1511		1492	
	1496		1583		1561							
			1491		1552							
					1520							
					1379							
	1328						1249			1304		856

Catalogue.—No. I. Stones and Irons.

Year.	Month and day.	Locality.	Spec. grav.	Iron or stone.	Remarks.
B.C. 1478	......	..............................Crete.	...	Stone.	?
1200	......	.........................Orchomenos.	...	? do.	
644	......	..............................China.	...	do.	
570 or 520	......	..............................Crete.	...	do.	
343 or 654	......	Rome.........................Italy.	...	do.	A shower of stones.
466	......	Egospotamos, Pergamus, Thrace.	...	do.	Very large stone.
204	......	Ancona.......................Italy.	...	do.	A shower.
211	......	..............................China.	...	do.	
192	......	..............................China.	...	do.	Two falls.
176	......	Crustumerian Territory......Italy.	...	? do.	
		Vocontii Territory............ Gaul.	...	do.	Time of Pliny.

Catalogue.—No. I. (*continued*).

Year.	Month and day.	Locality.	Spec. grav.	Iron or stone.	Remarks.
B.C. 89	......	.... China.	...	Stone.	
38 to 6	......	.... China.	...	do.	7 distinct falls.
46	......	Acilla........................ Africa.	...	do.	Several stones.
A.D. 2 to 333	......	.... China.	...	do.	5 distinct falls.
452	......	.... Thrace.	...	do.	3 large stones.
		Emessa and Mount Lebanon, Syria.	...	do.	6th century (?).
About 570	......	Bender .... Arabia.	...	do.	A shower.
616	......	.... China.	...	do.	Several.
823	......	.... Saxony.	...	do.	
856	Winter (Dec.)	.... Egypt.	...	do.	5 stones.
886 or 839	......	.... Japan.	...	? do.	?
892 or 897	......	Ahmendabad................. India.	...	do.	
921	......	Narni......................... Italy.	...	do.	A very large one.
905	......	.... China.	...	do.	
951	......	Augsburg................. Bavaria.	...	do.	One.
998	......	Magdeburg................Prussia.	...	do.	Two.
1021	July or Aug.	.... Africa.	...	do.	Several.
1057	......	Hoanglie................. China.	...	do.	
1112	......	Aquileia..................... Trieste.	...	? do.	
1198	......	Near Paris.................. France.	...	do.	
1135 or 1136	......	Oldisleben, Thuringia...Germany.	...	do.	One 12 in. in diam.
1164	......	Misnia......................Saxony.	...	do.	A shower.
?	......	Würzburg............ Franconia.	...	do.	13th century.
?	......	Welixos, Ussing............ Russia.	...	do.	13th or 14th century.
1249	July 26	Quedlinbourg............... Saxony.	...	do.	A shower.
1280	......	Alexandria.................. Egypt.	...	do.	One. (?)
About 1300	......	Arragon.......................Spain.	...	do.	Several large ones.
1304	Oct. 1	Friedland, Saale............ Saxony.	...	?	
1305	......	Vandals.................. S. Austria.	...	?	
1328	Jan. 9	...... In Mortahiah and Dakhalia.	...	do.	
1368	......	Oldenburg............... Germany.	...	Iron.	?
1379	May 26	Minden......................Hanover.	...	Stone.	
1421	......	.... Island of Java.	...	do.	One.
1438	......	Burgos......................... Spain.	...	do.	Many. ?
1474	......	Viterbo........................ Italy.	...	do.	Two large ones.
1480	......	.... S. Saxony or Bohemia.	...	do.	
1491	Mar.22	Crema...........................Italy.	...	do.	One.
1492	Nov. 7	Ensisheim.................. France.	3·50	do.	270 lbs. weight; one.
1496	Jan. 28	Cesena, Romagna............ Italy.	...	do.	(Doubtful).
1510	......	Padua........................... Italy.	...	do.	Large number.
1511	Sept.	Crema................. North Italy.	...	do.	6 fragments.
1516	......	.... China.	...	do.	
1520	May	Arragon.......................Spain.	...	do.	
About 1545	......	Neuhof..................... Saxony.	...	Iron.	
1545	......	Piedmont..................... Italy.	...	Stone.	?
1540	Apr. 28	Limousin....................France.	...	do.	?

Catalogue.—No. I. (*continued*).

Year.	Month and day.	Locality.	Spec. grav.	Iron or stone.	Remarks.
1552	May 19	Thuringia.................. Saxony.	...	?Stone.	
1559	......	Miscoz.............. Transylvania.	...	do.	Several.
1561	May 17	Eilenborg, Torgau ....Prussia.	...	do.	One.
1580	May 27	Göttingen (?)............ Germany.	...	do.	Several.
1581	July 26	Thuringia.................Germany.	...	do.	One.
1583	Mar. 2	Piedmont.................... Italy.	...	do.	
1585	Jan. 9	Castrovillari or Rosas ?...... Italy.	...	do.	30 lbs. ; one stone.
1591	June 9	Kumersdorf ?............ Germany.	...	do.	
1596	Mar. 1	Crevalcore.............. Piedmont.	...	do.	
1618	August	Muraköz.................... Styria.	...	do.	3 of about 100 lbs.
1620	Apr. 17	Jalindher.................... Persia.	...	Iron.	7 lbs. [each.
1622	Jan. 10	Devon.................... England.	...	Stone.	
1627	Nov.27	Provence.................... France.	...	do.	59 lbs.
1628	Aug. 9	Berkshire.................. England.	...	do.	
1634	Oct. 27	Charollois.................. France.	...	do.	Two stones. [S.
1635	Jun.21	Vago near Verona............ Italy.	...	do.	A large stone, N. to
1635	July 7	Calce, Vicenza............... Italy.	...	do.	11 oz. (Doubtful.)
1636	Mar. 6	Sagau, Silesia............... Prussia.	...	do.	One large one.
1639	Nov.29	Mt. Vaison, Maritime Alps, France.	...	do.	38 lbs. *One.*
1642	Aug. 4	Suffolk co., between Woodbridge and Aldboro'........ England.	...	do.	4 lbs.
1647	Feb. 18	Zwickau.................... Saxony.	...	do.	
1647	August	Stolzenau, Westphalia, Germany.	...	do.	?
1650	Aug. 6	Dordrecht..................Holland.	...	do.	One stone.
1650	Sep. 4 ?	Milan........................ Italy.	...	do.	
1654	Mar.30	Funen Island............ Denmark.	...	do.	A shower.
1668	Jun. 20	Verona........................ Italy.	...	do.	Large ones.
1671	Feb. 27	Swabia...................... Austria.	...	do.	A shower.
1673	......	Dietting.................... Bavaria.	...	do.	
1674	Oct. 6	Glarus canton.........Switzerland.	...	do.	
1676	......	Orkneys.................. Scotland.	...	do.	Fell into a boat.
1677	May 28	Ermendorf.................. Saxony.	...	do.	Several.
1680	May 18	Near London............ England.	...	do.	Several.
1683	Jan. 12	Castrovillari, Calabria...... Italy.	...	do.	
1683	Mar. 3	Piedmont.................... Italy.	...	do.	
1692	......	Temesvar.................Hungary.	...	do.	
1697	Jan. 13	Near Sienna.................. Italy.	...	do.	Several.
1698	May 19	Berne.................. Switzerland.	...	do.	
1700	Autum.	Jamaica.............. West Indies.	...	do.	
1715	Apr. 11	Garz, Pomerania......... Prussia.	...	do.	
1717	Jan.	Larissa.................. Macedonia.	...	do.	
1723	Jun. 22	Reichstadt.................. Bohemia.	...	do.	A shower.
1725	July 3	Mixburg, Northamptonsh., Engl.	...	do.	20 lbs.
1727	July 22	Lilaschitz..................Bohemia.	...	do.	Several.
1738	Aug.18	Carpentras.................. France.	...	do.	
1740	Oct. 25	Rasgrad.................... Hungary.	...	do.	Several.
1740– 1741	Winter	.... Greenland.	...	do.	A large stone.
1750	Oct. 12	Niort, Normandy......... France.	...	do.	A large stone.
1751	May 26	Agram.......................Croatia.	7·80	Iron.	71+16lbs. W.to E.
1752	June 5	Freisengen.................. Bavaria.	...	Stone.	Several (or 1722).
1753	July 3	Tabor...................... Bohemia.	3·65	do.	Several stones.
1753	Sept.	Liponas.................... France.	3·66	do.	Two = 31 lbs.
1755	July	Terra Nuova............ S. Italy.	...	do.	7 oz.
1766	July	Albereto near Milan...... S. Italy.	...	do.	One.

Catalogue.—No. I. (*continued*).

Year.	Month and day.	Locality.	Spec. grav.	Iron or stone.	Remarks.
1766	Aug.15	Novellara, Modena.........S. Italy.	...	Stone.	Doubtful.
1768	Sep. 13	Lucé......................... France.	3·50	do.	7½ lbs.
1768	Nov.20	Manerkirchen............ Bavaria.	3·45	do.	Two ; one of 38 lbs.
1773	Nov.17	Sigena, Arragon............ Spain.	3·63	do.	9 lbs.
1775	Sep. 19	Rödach, Coburg......... Germany.	...	do.	
1775 or 1776	......	Obruteza in Volhynia...... Russia.	...	do.	
1776 or 1777	......	Fabriano, Ancona............ Italy.	...	do.	
1779	......	Pettiswood, West Meath, Ireland.	...	do.	6 oz.
1780	Apr. 1	Beeston....................England.	...	do.	?
1782	......	Turin........................... Italy.	...	do.	One fell.
1785	Feb. 19	Eichstadt.................. Bavaria.	3·65	do.	
1787	Oct. 1	Khartof, Ukraine......... Russia.	...	do.	A shower.
1789	Aug.20	..............,.......... France.	...	do.	15 inches diameter.
1790	July 24	Barbotan....................France.	3·62	do.	Several of 10 and
1791	Oct. 20	Menabilly, Cornwall... England.	...	do.	[20 lbs.
1791	May 17	Tuscany........................ Italy.	...	do.	
1794	June16	Sienna.......................Italy.	3·40	do.	12 small ones.
1795	Dec. 13	Wold Cottage, Yorksh., England.	3·85 3·55	do.	56 lbs.
1795	Apr. 13	Ceylon....................... India.	...	do.	
1796	Jan. 4	Bélaja, Zerkwa............ Russia.	...	do.	
1796	Feb. 19	Friexo....................Portugal.	...	do.	10 lbs.
1796	Mar. 8	Lusatia..................... Saxony.	...	do.	
1798	Mar.12	Salis.......................France.	3·45	do.	Or Mar. 8. W. to E.
1798	Dec. 13	Benares....................... India.	3·36	do.	A shower.
1799	Apr. 5	Baton Rouge, Mississippi... U.S.	...	do.	Belfast Chron. of
?1802	Sept.	........................Scotland.	...	do.	Several. [the War.
1803	Oct. 8	Apt, Provence.............France.	3·48	do.	7 lbs.
1803	July 4	East Norton, Leicestersh.... Engl.	...	do.	
1803	Dec.13	L'Aigle.................... France.	3·45	do.	3000 stones fell.
1803	Dec.13	Mässing.................... Bavaria.	3·26	do.	3¼ lbs. Contains little iron.
1804	Apr. 5	Possil, Glasgow......... Scotland.	3·53	do.	S.E. to N.W.
1805	Mar.25	Irkutsk.................... Siberia.	...	do.	*Two*, of 7+2¼ lbs.
1805	June	Constantinople............ Turkey.	3·17	do.	Contains no iron.
1805	Nov.	Asco...................... Corsica.	3·66	do.	[iron.
1806	Mar.15	Alais....................... France.	1·70	do.	Carbonaceous ; no
1806	May 17	Glastonbury, Somerset...England.	...	do.	*One*, 2¼ lbs.
1807	Mar.13	Timochin, Smolensk... Russia.	3·64	do.	160 lbs.
1807	Dec. 14	Weston, Connecticut... U. States.	3·50	do.	300 lbs. ; in frag-
1808	......	Moradabad.................. India.	...	do.	[ments.
1808	Apr. 19	Parma........................Italy.	3·40	do.	[iron.
1808	May 22	Stanneru.................. Moravia.	3·15	do.	250 stones fell ; no
1808	Sept. 3	Lissa.......................Bohemia.	3·52	do.	4 or 5 small ones.
1809	......	Kikina, Smolensk......... Russia.	...	do.	[board.
1809	......	Lat. 30° 58′ N., long. 70° 25′ W.	...	do.	6 oz. Fell on ship-
1810	Jan. 7	Caswell, N. Carolina......... U.S.	...	do.	3 lbs.
1810	July	Futty-Ghur................. India.	...	? do.	
1810	August	Tipperary.................. Ireland.	...	do.	7¾ lbs.
1811	Nov.23	Panganoor.................... India.	...	Iron(?)	[13 lbs.
1811	Mar.12	Poltawa.................... Russia.	3·49	Stone.	*Two* fell ; one of
1811	July 8	Berlanguillas................Spain.	3·49	do.	3 fell.
1812	Apr. 12	Toulouse.................... France.	3·70	do.	Several small ones.

Catalogue.—No. I. (*continued*).

Year.	Month and day.	Locality.	Spec. grav.	Iron or stone.	Remarks.
1812	Apr. 15	Erxleben....................Saxony.	3·63	Stone.	4½ lbs. [2 smaller.
1812	Aug. 5	Chantonnay..................France.	3·46	do.	One of 69 lbs., and
1813	Mar.14	Cutro, Calabria.............. Italy.	...	do.	Dust and stones.
1813	Sep. 10	Limerick.....................Ireland.	3·64	do.	17+65+24 lbs. E. to W.
1813	Dec. 13	Lontalex, Wiberg......... Finland.	3·07	do.	Contains no iron.
1814	Feb. 1	Bachmut, Ekaterinoslaw, Russia.	3·42	do.	40 lbs.
1814	Sept. 5	Agen........................ France.	3·60	do.	Several; *one* of 18
1814 or 1812	......	Saros.............. North Hungary.	...	do.	112 lbs. [lbs.
1815	Feb. 18	Loodianah.................... India.	...	do.	25 lbs.
1815	Oct. 3	Chassigny.................. France.	3·65	do.	8 lbs.; contains no
1816	......	Near Nagy Banya...... Hungary.	...	do.	[iron.
1818	Mar.30	Gov. of Volhynia............Russia.	3·40	do.	One.
1818	Feb. 15	Limoges..................... France	...	? do.	?
1818	June	Seres...................Macedonia.	3·70	do.	15 lbs.
1818	Aug.10	Slobodka, Smolensk...... Russia.	3·47	do.	*One.*
1819	Jun. 13	Jonzac......................France.	3·08	do.	Contains no iron.
1819	Oct. 13	Politz, Gera............ S. Prussia.	3·39	do.	3 fell; *one* 7 lbs.
1820	July 12	Lixna, Witepsk............ Russia.	3·70	do.	14½ lbs.
1820	Mar.21	Vedenberg.............. Hungary.	...	do.	?
1820	Nov.29	Cosenza, Calabria............ Italy.	...	do.	A shower of stones.
1821	Jun. 15	Juvenas.................... France.	3·10	do.	3 fell; one 220 lbs. Contains only 1·5 iron.
1822	Jun. 9	Angers......................France.	...	do.	
1822	Sep. 10	Carlstadt.................. Sweden.	...	do.	
1822	Sep. 13	La Baffe, Vosges............France.	3·66	do.	[Dir. S.E. to N.W.
1822	Nov.30	Futtehpore, Doab............ India.	3·35	do.	Several; 1 of 22 lbs.
1823	Aug. 7	Nobleboro', Maine............ U.S.	3·09	do.	16 lbs. (sp.gr. 2·0)?
1824	Jan. 15	Renazzo....................... Italy.	3·25	do.	3 small ones fell.
1824	Oct. 14	Zebrak.................... Bohemia.	3·60	do.	4 lbs.
1824	Feb. 18	Irkutsk.................... Siberia.	...	do.	5 lbs.
1825	Jan. 16	Oriang, Malwate............ India.	...	do.	
1825	Feb. 10	Nanjemoy, Maryland......... U.S.	3·66	do.	16 lbs. [30 lbs.
1825	Sep. 14	Owhyhee......... Sandwich Isles.	3·39	do.	Two fell; together
1825	......	Ekatherinosloff............ Russia.	3·77	do.	86 lbs.
1826	Sept.	Waterville, Maine......... U.S.	...	do.	Doubtful. [pounds.
1827	Feb. 27	Mhow, Ghazeepore......... India.	3·5	do.	*One,* of several
1827	Oct. 5 or 8	Bialistock....................Russia.	3·17	do.	4 lbs.; contains no [iron.
1827	May 9	Nashville, Summer co., Tennessee, U.S.	3·55	do.	3 fell, one 5 lbs., [another 11½ lbs.
1828	Jun. 4	Richmond, Virginia............ U.S.	3·34	do.	4 lbs.
1829	Sept. 9	Forsythe, Georgia............ U.S.	3·50	do.	36 lbs.
1829	Aug.15	Deal, New Jersey................U.S.	...	do.	
1830	Feb. 15	Launton, Oxford......... England.	...	do.	2½ lbs.
1831	July 18	Poitiers, Vouillé............ France.	3·55	do.	40 lbs. (or May 13).
1833	Nov.25	Blansko....................Moravia.	...	do.	8 lbs.
1833	Sept. 9	Wessely....................Moravia.	...	do.	
1833	Dec.28	Okaninak, Volhynia...... Russia.	...	do.	30 lbs.(27or29Dec.)
1834	Jun. 12	Charwallas....................India.	3·38	do.	7 lbs.
1834	?Nov29	Raffaten, borders of Hungary and Wallachia.	...	do.	A shower.
1835	July 30	Dickson co., Tennessee...... U.S.	...	Iron.	9 lbs.

Catalogue.—No. I. (*continued*).

Year	Month and day.	Locality.	Spec. grav.	Iron or stone.	Remarks.
1835	Aug. 4	Cirencester.............. England.	...	Stone.	2 lbs.
1835	Nov.13	Simond, de l'Ain...........France.	1·35	do.	Contains no iron.
1836	Dec.11	Macao....................... China.	3·72	do.	Immense shower.
1836	......	Platten See.............. Hungary.	...	do.	
1837	July 24	Gross-Divina........... Hungary.	3·55	do.	19 lbs.
1837	August	Esnaude, Charente......... France.	...	do.	3 lbs.
1838	Apr. 18	Akburpoor....................India.	...	do.	4 lbs.
1838	June 6	Chandakapore, Berar...... India.	3·53	do.	3 fragments.
1838	Oct. 13	Cold Bokkewelde, Cape of Good Hope.	2·69	do.	Many. N.W.to S.E. Carbonaceous.
1839	Feb. 13	Little Piney Point, Missouri, U.S.	...	do.	50 lbs. N.E.to S.W.
1839	Nov.29	Naples............................Italy.	...	do.	
1840	July 17	Casale.................... Piedmont.	...	do.	11 lbs.
1840	May 9	Kirghiz Steppes...........Tartary.	...	do.	
1841	Mar.22	Grünberg....................Silesia.	3·72	do.	2½ lbs.
1841	Jun. 12	Chateau Renard, Loiret... France.	3·54	do.	75 lbs.
1841	August	Iwan..........................Hungary.	...	do.	(Or beginning of
1841	Nov. 5	La Vendée................. France.	...	do.	11 lbs. [Sept.)
1842	Apr. 26	Milena, Croatia........... Austria.	3·54	do.	
1842	July 4	Logrono.......................Spain.	...	do.	7 lbs.
1843	Mar.25	Bishopville, S. Carolina...... U.S.	3·02	do.	13 lbs.; contains no
1843	June 2	Utrecht.................... Holland.	...	do.	[iron.
1843	July 26	Manegon, Khandeish...... India.	...	do.	10 in. in diam.
1843	Sep. 16	Kleinwinden, Mulhausen, Germ.	...	do.	
1844	Apr. 29	Killeter, co. Tyrone...... Ireland.	...	do.	
1844	Oct. 2	St. Andrew's................. Cuba.	...	do.	Doubtful.
1846	May 10	Macerata, Monte Milone... Italy.	...	do.	(Or May 8.) 9
1846	Nov.11	Lowell, Massachussets...... U.S.	...	do.	[stones.
1846	Dec.25	Minderthal.............. Germany.	...	do.	6 lbs.
1846	Summ.	Richland, S. Carolina........ U.S.	2·32	do.	6 oz.
1847	Feb. 25	Iowa, Linn. co............... U.S.	3·58	do.	75 lbs.
1847	July 14	Braunau..................... Silesia.	7·71	Iron.	Two fragments, 42
1848	Feb. 15	Dharwar..................... India.	3·50	Stone.	4 lbs. [and 30 lbs.
1848	May 20	Castine, Maine...................U.S.	3·45	do.	1½ oz. S.E. to N.W.
1849 or 1850	......	Tunis.................... N. Africa.	...	do.	A shower.
1849	Oct. 31	Cabarras co., N. Carolina... U.S.	3·63	do.	18 lbs.
1849	Mar.19	Poonah....................... India.	...	do.	
1850	Nov.30	Bissempore.................. India.	...	do.	3 feet in diameter.
1851	April	Guterlof.............. Westphalia.	...	do.	
1852	Sep. 4	Mezo-Madaras...... Transylvania.	3·50	do.	18 lbs. S.W.to N.E.
1853	Feb. 10	Girgenti.........................Sicily.	3·76	do.	A large stone.
1854	Sep. 5	Fehrbellin, nr. Potsdam, Germany.	...	do.	6 lbs.

Catalogue.—No. II.   Iron Meteoric Masses.

Year found.	Locality.	Spec. grav.	Pounds weight.	Remarks, peculiarities, &c.
B.C. 1168	.... Crete.	...	......	?
1168	Mount Ida....................Crete.	...	......	? [mass.
52 or 56	Lucania.................... S. Italy.	...	......	? A spongy or vesicular

Catalogue.—No. II. (*continued*).

Year found.	Locality.	Spec. grav.	Pounds weight.	Remarks.
A.D.				
1368	Oldenburg.............. Germany.	...	......	? Iron; fell in 1368.
1545	Neuhof.................... Saxony.	...	......	Fell between 1540 & 1550.
1618	..........................Bohemia.	...	......	Fell 1618. ?
1620	Jalindher.................... Persia.	...	7	Fell 1620, April 17.
1712	Krasnojarsk.................Siberia.	6·48	1,600	Cont. crystallized olivine.
1717	Senegal................. W. Africa.	7·72	......	Large quantity. Has crystalline structure.
1751	Agram.......................Croatia.	7·80	71+16	Two fragments; shows Widmänstattian figures when polished.
1780	Lahore ....India.	...	......	Fell 1780.
1783	(Tucuman), Otumpa ......Mexico.	...	......	Wid. figures, very perfect.
1784	Rio de la Plate.........S. America.	7·60	30,000	Crystalline structure imperfect.
1784	Ziquipilco, Toluca......... Mexico.	7·67	......	Large quantity. Shows Wid. figures.
1784	Sierra Blanca...............Mexico.	...	4,000	do.
1792	Zacatecas................. Mexico.	7·50	2,000	Does not show Wid. figs.
1793	Cape of Good Hope... S. Africa.	7·00	......	Plenty; do. do.
1805	Bitberg..................... Prussia.	6·50	3,400	Wid. figs.; ? with olivine.
1808	Texas, Red River...............U.S.	7·70	3,000	do., very distinct.
1810	Brahin........................ Russia.	6·20	200	With crystalline olivine.
1811	Panganoor..................... India.	...	......	Fell 1811; ? iron.
1811	Elbogen................. Bohemia.	7·74	190	Shows faint Wid. figures.
1811	Durango.................... Mexico.	7·88	35,000	Wid. figures, distinct.
1810	Rasgata, Santa Rosas, N. Granada.	7·30	1,700	Vesicular and malleable.
1814	Lenarto................. Hungary.	7·75	194	Wid. figs., very distinct.
1816	White Mountains, Franconia, New Hampshire.................. U.S.	...	20	
1818	Lockport, New York......... U.S.	...	36	Wid. figs.; cont. pyrites.
1819	Burlington, Otsego, N. York, U.S.	7·50	150	do., extremely hard.
1819	Baffin's Bay............. Greenland.	7·23	......	Large mass.
1820	Guildford, N. Carolina...... U.S.	7·67	28	[texture very hard.
1822	Randolph co., N. Carolina... U.S.	...	2	Crystalline structure faint;
1827	Atacama..................... Bolivia.	...	300	With crystallized olivine.
1828	Caille, Departm. du Var, France.	7·64	1,100	Known 200 years ago. Wid.
1828	Bedford co., Pennsylvania... U.S.	6·91	......	(Doubtful mass.) [figs.
1829	Bohumilitz.............. Bohemia.	7·60	103	With Schreibersite.
1823 or 1824	Kinsdale, between West Mountains and Connecticut......U.S.	...	......	Several pieces.
1832	Walker co., Alabama......... U.S.	7·26	165	Has no crystalline structure.
1834	Scriba, Oswego co., N. York, U.S.	...	8	Wid. figures. [ture.
1834	Claiborne co., Alabama...... U.S.	6·5	20	
1835	Dickson co., Tennessee...... U.S.	...	9	Fell July 30.
1835	Black Mountains, Buncombe co., N. Carolina.................. U.S.	7·26		
1839	Asheville, Buncombe co., N.C., U.S.	7·90	30	
1839	Putnam co., Georgia......... U.S.	7·69	70	Wid. figures.
1840	Cocke co., Tennessee......... U.S.	7·26	2,000	Crystallized in structure, with graphite & magnetite.
1841	Petropawlawski............ Siberia.	7·76	17	Found 31 feet in the soil.
1841	Newberry, 'Ruff Mountains,' South Carolina... .... U.S.	7·10	117	Structure crystalline.
1842	Greene co., 'Babb's Mill, Ten.,' U.S.	...	12+6	*Two.* No Wid. figures.

Catalogue.—No. II. (*continued*).

Year found.	Locality.	Spec. grav.	Pounds weight.	Remarks.
1843	Otsego co., New York.........U.S.	...	276grs.	Finely crystalline.
1843	St. Augustine's Bay, Madagascar.	...	......	Large quantities.
1843	Arva........................ Hungary.	7·1	......	Contains graphite and schreibersite.
1845	Buncombe co., Hommoney Creek, N. Carolina.................. U.S.	7·32	27	Vesicular, and with a crystalline structure.
1845	De Kalb co., Tennessee...... U.S.	...	36	
1846	Jackson co., Tennessee...... U.S.			
1846	Smithland, Tennessee......... U.S.			
1847	Chester co., S. Carolina...... U.S.	...	......	No Wid. figures.
1847	Seeläsgen.....................Silesia.	7·70	218	do.
1849	Fort Singhur, Deccan...... India.	4·80	31	Olivinoid and vesicular.
1850	Schwetz....................... Prussia.	7·77	43	Wid. figures, distinct.
1850	Pittsberg, Pennsylvania...... U.S.	7·38		
1853	Tazewell,Claiborne co.,Tenn.,U.S.	7·30	60	Has a crystalline structure.
1853	Long Creek, Jefferson co., Tennessee....................... U.S.	7·43	3	
Unknown.	Haywood co., N. Carolina... U.S.	7·42	½ oz.	Crystalline in structure.
	Lead Hills............... Scotland.	...	¾ oz.	Finely crystalline ; very hard.
	Potosi,................. S. America.	...	......	? Atacama iron. [hard.
	Steinbach................. Saxony.	...	......	With olivine.
	Seneca River, Cayuga co., New York............................U.S.	7·34	9	With Wid. figs. & pyrites.
	Lion River............... S. Africa.	...	170	
	Oaxaca...................... Mexico.	7·38	......	Shows Wid. figures.
	Salt River, Kentucky......... U.S	6·83	......	Doubtful if meteoric.
	Murfreesboro', Tennessee... U.S.	...	......	Large mass.
	Charlotte co., Tennessee......U.S.			
	Grayson co., Tennessee...... U.S.			
	Roanoak, Virginia........... U.S.			
	Alasej Mountains......... Siberia.	...	......	Large quantity.
	Tucson, Sonora...... New Mexico.	8·13		
	Livingston co., Kentucky... U.S.			
1847	Braunau..................... Silesia.	7·71	42+30	Two fragments. Wid. figs.; very small. Cont. pyrites. Fell July 14, 1847.
	Near the Caspian Sea...............	...	......	Contains iron, nickel, cobalt and copper.

Catalogue.—No. III.   Doubtful.

Year.	Locality.		Remarks.
?	Daghistan............... (? Scythia).	Stone.	Fell B.C.
648	Constantinople	do.	
1095	.................................France.	do.	April 4.
1672	.................................France.	?	
1676	Copinsha................. ?	?	
1676	Near Leghorn..................Italy.	?	March 21. Fell in the sea.
1753	Eichstadt.................. Germany.	Stone.	January. ?
1756	.................................France.	do.	
1776	Novellara.................... Italy	do.	August 5.

Catalogue.—No. III. (*continued*).

Year.	Locality.		Remarks.
1783	.... England.	Shower of st.	August 18.
1785	...............................France.	?	
1799	Baton Rouge, Mississippi... U.S.	?	April 5.
1805	Dordrecht.................. Holland.	Stone.	
1806	Basingstoke, Hants...... England.	do.	May 17.
1810	.............................France.	do.	?
1809	.... South Atlantic.	?	Fell into the sea. June 19.
1814	Doab........................... India.	?	November 5.
1813	Malpas, Cheshire......... England.	Shower of st.	In the summer.
1817	Paris....................... France.	?	November 3.
1817	.... Baltic.	?	Fell in the sea. May 2.
1822	Kadonah, near Agra......... India.	Stone.	Aug. 7. Same as the fall at Futtehpore.
1824	Sterlitamak, Orenberg... Russia.	....	Hailstones, enclosing cry-
1826	Castres.......................France.	?	[stals of pyrites. Sept.
1826 or 1827	Waterloo, Seneca co., N.Y., U.S.	Stone.	Spec. grav. 2·30.
1827 or 1828	Alport, Derbyshire...... England.	do.	Autumn.
1801	Isle aux Tonneliers... Mauritius.	Stone.	
?	Pulrose.............. Isle of Man.	do.	
?	Concord, New Hampshire... U.S.	do.	
?	..............................Russia.	do.	Several. Dates unknown.
?	.... Affghanistan.	do.	(See *Comptes Rendus.*
?	Lucerne.............. Switzerland.	Dust.	? [1836.)
1637 1762 1814 1819	.... Canada	do.	Explosions, with meteors.
	Years 850, 1110, 1548, 1557, 1652, 1686, 1718, 1796, 1811, 1819 and 1844.	....	Falls of viscid matter, with luminous meteors.

LVII. *Contribution towards the History of Paraffine.*
By Baron REICHENBACH *.

NEARLY a quarter of a century has now elapsed since I exhibited the first specimen of paraffine to the German Association of Naturalists at Hamburgh in 1830, and described the mode of preparing this substance in Schweigger's Journal of Chemistry.   With the exception of some analyses by Ettling, Lewy, and others, very little has been communicated regarding this body since that period.   The very valuable properties of paraffine, the power with which it resists the action of concentrated acids and alkalies, and even of potassium at a boiling temperature, the brilliant whiteness of its flame, which deposits no soot, its beautiful translucency and its lubricating quality,

* From the *Journal für praktische Chemie*, by Otto Linné Erdmann and Gustav Werther, No. 17, October 1854.

recommend this substance for various technical applications; and I have myself called attention to this fact from its first discovery, without being able, however, to arrive at any satisfactory results in its industrial application. The difficulty which presented itself, was the small quantity which the dry distillation of wood furnishes of this substance. I subsequently showed that paraffine might be obtained from vegetable oils, from animal substances, and likewise from pit-coal; but from all these substances so minute a quantity was obtained that its production for manufacturing purposes was neither remunerative nor inviting. Thus paraffine remained up to this period an interesting curiosity, unknown except in the collection of scientific chemists.

I now hear that in England, which is essentially the country of useful applications, a manufactory of paraffine upon an extended scale has been carried on by Mr. Young since 1850. This talented chemist has succeeded in finding a method which yields comparatively large quantities of paraffine. Mr. Young obtains 13 pounds from a ton of cannel coal. This discovery renders the preparation of paraffine a lucrative branch of industry, especially since, in addition to the production of paraffine, a large quantity, about 30 gallons, of a lubricating oil is obtained which is saturated with paraffine, and which is said to surpass all other fatty substances as an antifrictional, and to have been already so generally adopted, that Young's works now supply weekly as much as 8000 gallons to the market. The most evident proof of the advantages offered by paraffine, is the rivalry which has already ensued in its production, and is well illustrated by an action at law, which some of the later competitors of Mr. Young have brought against him with the view of upsetting his patents, by attempting to prove that the discovery was not new, and was in fact not capable of being patented. It is true that the discovery of paraffine is my own, and I have announced it. To Mr. Young, however, belongs the merit of a second discovery, the merit of having elaborated a method which furnishes a comparatively *large* quantity of this substance, and which is sufficiently remunerative to the manufacturer; a result which I had vainly endeavoured to realize. I hope that Mr. Young will succeed in convincing the legal authorities of the priority of his practical discovery, which was not part of any purely scientific investigation, and which I cannot claim in any way. May he enjoy the fruits of his invention and of his industry, which he deserves, and to which no other person has a right!

LVIII. *On the Conductibility of Liquids for Electricity.* *By* MM. Van Breda *and* Logeman. *In a letter to* Professor Faraday.

Royal Institution,
Oct. 31, 1854.

My dear Tyndall,

I SEND the enclosed letter from MM. Van Breda and Logeman to you as an Editor of the Philosophical Magazine. If you should judge it proper for insertion in that Journal, I shall be very happy to see it there, but will beg you to accompany it on my part with the observation that it is not so conclusive in proving the negative (a thing very difficult to do) as to move me at present from the reserved condition of mind which I have recently expressed in respect of this matter.

Ever yours truly,
M. Faraday.

To Professor Faraday.

Sir,

The experiments on electro-dynamic induction in liquids which you have published in a letter to M. de la Rive, have excited our lively interest, not only because the phænomenon appeared to us to be of importance in itself, but especially because it seemed likely to throw some light upon the manner in which electricity is propagated in liquids.

Do liquids conduct exclusively by electrolysis, or do they also possess a proper conductibility, similar to that of metals? An experiment that we have made may perhaps assist in the solution of this question. It is well known that the conductibility of liquids increases with their temperature, whilst the opposite effect takes place with the metals, a fact which is easily explained if we suppose that liquids in general oppose less resistance to decomposition in proportion as their temperature is raised. If this explanation be the true one, the next thing to be ascertained is, whether a liquid will also exhibit this increase of conductibility for a current so weak as to traverse it without producing any apparent chemical decomposition. If this were the case, it would appear probable that the decomposition nevertheless took place, and that it was by its intervention that the current passed through the liquid. We have endeavoured to solve this question by the following experiment. We passed the current of a small Daniell's element through a column of distilled water 24 centimetres in length, contained in a glass tube of about 15 millimetres in diameter; the electrodes were of platinum wire. One of these electrodes was connected with the zinc pole of the battery, the other with one end of the helix of a galvanometer of which the

wire made 1800 coils, the other end of which communicated with the copper pole. The tube was immersed in a water-bath, the temperature of which could be raised by means of a spirit-lamp. When the water was at 59° F., the needle of the galvanometer deviated 4°. When the lamp was lighted, this deviation was seen to increase regularly. At a temperature of 152°·6 F., the deviation was 7°, and at 190°·4 F. it was 11°. The increase of the conductibility of the liquid by heat was therefore proved, even when traversed by an excessively feeble current. Had any chemical decomposition of the water taken place during this experiment? Its direct result led to the belief that such was the case, but we were also fortified in this opinion by the following circumstances. When the liquid was cooled, the communications remaining untouched, the needle of the galvanometer no longer showed any appreciable deviation. When the direction of the current in the column of water was reversed, the needle immediately deviated 8° and returned insensibly, but in a short time to 4°, at which point it remained stationary. It was consequently an effect of the polarization of the electrodes that we observed in this case, a polarization which opposed the current at the first moment of its passing, without, however, being able to annul it, but which annulled it completely when it had become stronger by the passage of the stronger current through the heated liquid.

But is this polarization the peculiar effect, and consequently the irrefragable proof, of chemical action? There are many experiments which render this opinion, if not absolutely certain, at least exceedingly probable. We may mention in particular those of Schönbein, who found that the effect continues when the electrodes which have served to introduce a current into a liquid are immersed in another liquid through which no current has been passed, and also that effects exactly similar to those of the plates polarized by the current may be obtained by putting one of them only in contact with a gas (such as hydrogen or chlorine) for a very short time, and afterwards immersing them in acidulated water[*]. Some physicists, however, still maintain the opposite opinion. They explain polarization by an accumulation of electricity of different natures, either in the electrodes themselves, or in the adjacent portions of the liquid; these two electricities in recombining by a conductor uniting the two electrodes, after the connexion between these and the electromotor has been broken, would give rise to the current in the opposite direction to that of the latter, which is always observed in such cases.

It appeared to us that your *beautiful experiment on electro-*

[*] Poggendorff's *Annalen,* vol. xlvi. p. 109, and vol. xlvii. p. 101.

dynamic induction in liquids might furnish a means of submitting this opinion to an experimental test, by trying whether the electrodes, employed in that experiment to conduct the instantaneous current of the fluid helix to the galvanometer, are or are not polarized by this current.   To obtain a decisive effect it was necessary to reproduce the phænomenon with more intensity than when, as in your experiments, the question was merely to prove the phænomenon itself.   For this purpose we made use of a tube of vulcanized Indian rubber, of about 1 centimetre in internal diameter and 13 metres in length.   We twisted it round the two branches of the large electro-magnet intended for experiments in diamagnetism, which, if we are not mistaken, has the same form and the same dimensions as your own; it is covered by a coil of copper wire 3 millimetres in diameter and 180 metres long.   The tube was entirely filled with a mixture of 6 parts by volume of water and 1 part of sulphuric acid.   It was terminated at both ends by glass tubes of about 4 centimetres in length; into each of these passed a platinum wire of 1 millimetre in diameter, the portion of which immersed in the liquid was about 2·5 centimetres in length.   All being thus arranged, the ends of the two platinum wires were connected with the galvanometer of 1800 coils which was placed at a distance of 10 metres.   We had ascertained previously that at this distance the magnet did not exercise any sensible action upon the needles.   The moment the two ends of the copper-wire coil of the electro-magnet were put in connexion with the poles of a Grove's battery of 60 large elements, arranged in a double series of 30, the needle of the galvanometer deviated suddenly about 40°, and returned, after oscillations which occupied between 1 and 2 minutes, to 0°. When the circuit of the pile was interrupted, the galvanometer deviated again about the same number of degrees, but in the opposite direction, returning again to 0° in the same manner. The needle returning in both cases to 0°, one would be tempted, at first sight, to think that there was no polarization of the electrodes.   But the strong impulsion communicated to the very astatic system of the galvanometer by the induced current, causing the needles to oscillate during a considerable period as we have just stated, the circuit remaining always complete, it appeared possible that the polarization, if it existed, had already exhausted itself before the needles had arrived at a state of repose. To get rid of this difficulty we put the two electrodes in direct communication with each other by means of a copper wire of only 10 centimetres in length, although they still remained in connexion with the galvanometer.   The induced current produced when the circuit of the pile was established, then passed by this wire rather than by the infinitely longer wire of the

galvanometer, and the needles remained at rest.   But when this wire was removed after the establishment of the communication with the pile, *we saw the galvanometer deviate instantly in an opposite direction to the deviation produced by the induced current of the preceding experiment, and rest, after a few oscillations, at* 10°.   *By replacing the wire, then interrupting the circuit and again removing the wire, we saw the galvanometer deviate in the contrary direction, and rest, in the same manner, at about* 10° *on the other side of the divided arc.*   In both cases the deviation diminished regularly by little and little until it became 0° ; we did not exactly measure the time which this occupied, but it appeared to us to be about 30 or 40 seconds.

These experiments were frequently repeated, and always with the same result.   We need not say that we always took the precautions pointed out by you, to prevent the effect of an induction in one of the metallic conductors.

Polarization therefore takes place in the electrodes which serve, not only to convey a current into a liquid, but to carry out the current induced in the liquid itself.   It appears to us that this fact directly contradicts the theory which attributes polarization to an accumulation of the two electricities upon or around the electrodes ; for in the present case not only would such an accumulation be infinitely less probable than in ordinary cases of polarization, but if it existed, it would necessarily give rise to a current not in a direction opposed to that of the principal current, but in the same direction.

May we therefore regard all polarization as an effect of electrolytic decomposition, and consequently as an irrefragable proof of the existence of this decomposition ?   If this be true, we shall be led to regard the opinion of those who admit the possibility of the transmission of a current, or of a portion of a current through a decomposable fluid without the occurrence of any decomposition, as resting upon very slight grounds.   Whenever we have passed a current, however weak, through such a liquid, we have always observed an undoubted polarization of the electrodes.

We shall take the liberty to describe one other experiment, which is still more convincing in this respect than that described at the commencement of this communication.   We immersed two plates of platinum, 6 centimetres in length and 5 centimetres in breadth, at a distance of about 1 centimetre from each other, in distilled water.   One of these plates communicated with the ground by a metal wire ; they had previously been carefully cleaned by heating to redness, and consequently, when put in communication with the galvanometer, did not produce any sensible deviation.   But as soon as *a single spark* from a common

electrical machine had been thrown upon the plate which did not communicate directly with the ground, and the communication with the galvanometer had been established, the needle deviated from $3°$ to $4°$ in one direction, and the same distance in the opposite direction when the current of the spark was passed through the water the other way. This deviation could be brought to $15°$ or more by throwing several sparks instead of one upon one of the plates, or by connecting the plate for a very short time with the conductor of the machine during the movement of its plate.

We fear that the importance of this letter will not be proportional to its length; if, however, its contents should seem to you to be worthy of attention, we shall be happy to see it published in any manner you think proper.

We remain, Sir, &c.,

<div align="right">

J. G. S. Van Breda.
W. M. Logeman.

</div>

Haarlem, September 1854.

LIX. *On the Changes of Wind in a Cyclone. In a Letter from* Professor Dove *of Berlin to* Captain Washington, *R.N., F.R.S.*

<div align="right">

13 Ashley Place, Westminster,
September 12, 1854.

</div>

Dear Sir,

IN our conversation yesterday on the subject of cyclones, I made the remark that too wide an extension was given to the theory respecting them when all changes of wind in a particular direction were referred to them, whereas an essential distinction ought to be made between those changes of wind which are produced by the advance of a cyclone over the surface of the earth, and those which are the necessary consequences of the phænomenon which I have called the law of deflection. Permit me to express my meaning in rather more detail.

When a cyclone is moving over the earth's surface in any direction, the place of observation may be in either of two cases. The middle or centre of the cyclone may, in passing, be either over it or on one side of it. In the first case, the place is in a diameter; and in the second case, it is in a chord of the rotatory storm or cyclone. In both cases the observer sees the wind-vane as a tangent of the cyclone; only with the difference, that if it is the centre of the storm which passes over him, two winds from directly opposite directions succeeding each other are separated by an interval of calm; whereas if, instead of being on the diameter, he is on a chord at some distance from the centre, he experiences no such interval of lull, but finds in its place a more or less prolonged time during which the wind veers round. The direction in which it does so is always opposite on

the two opposite sides of the middle line. In a cyclone advancing towards the north (in the northern hemisphere), the change of wind on its eastern side always takes place from E. through S. to W., or "with the sun;" on the western side of the storm, from E. through N. to W., or "against the sun." Now if all changes of wind were more or less direct results of advancing rotatory storms or cyclones, there would necessarily be found in the northern hemisphere as many cases of change of wind of one of these ways as in the other; for although the cyclones prevail and pass over certain parts of the ocean more especially, yet the preponderance of one direction of the change of wind in one place would be compensated by the opposite preponderance in another, and on the whole there would not remain any preponderance.

I have, however, shown by special investigations, that there is such a preponderance, and that it is the case in both hemispheres; that in the non-tropical zone of the northern hemisphere the wind in the majority of cases changes from S. through W. to N. and E., and in the southern hemisphere from S. through E. to N. and W.; this demonstrated preponderance must therefore have some other cause, and that cause must be one of general conditions, because it produces opposite effects in opposite hemispheres. The space between two meridians is an equilateral triangle, having its base on the Equator and its summit at the Pole. It is evident that the whole mass of air which ascends over the base line in the meridian of calms cannot flow in the rapidly narrowing space up to the point at the summit of the triangle, but that it must ascend again before reaching that point.

This descent takes place at the outer limit of the "trade winds," but at variable points; so that while at certain places portions of air are flowing from the upper regions of the tropical atmosphere into the temperate zone, at other places at the same time the contrary may be taking place, causing an extension backwards of the "trade wind," and making it appear to begin earlier than would correspond to its *mean* outer limit.

The currents of air which in the tropics flow *over* each other, in the temperate zones flow *alongside* each other and in variable beds. Our phænomena of weather, therefore, at each particular place of observation depend on two currents, which alternate as each in turn prevails over and presses aside the other, or drives it off the field. When one of these currents prevails in full intensity, we have the extremes of climate; where they strive for the mastery, we have its variability. Let us now suppose that in London a northerly current of air begins, that is to say, that the mass of air between London and a point to the north of the Shetlands is set in motion; an observer in London will remark

first a north wind, but the further north the place from whence this north wind comes the more will it change into a N.E. wind, as the velocity of rotation of the earth is less in the higher than in the lower latitudes; therefore the wind that left Edinburgh as a N. wind reaches London, we will say, as a N.N.E. wind; if it left the latitude of Shetland as a N. wind, it reaches London as a N.E. wind; and if it left the polar circle as a N. wind, it reaches London as an E.N.E. wind. It is plain that in these cases we have only to do with one unaltered current, which, the further it proceeds, the more its original direction appears deflected. Winds between N. and E. are therefore, properly speaking, N. winds; in other words, a N.E. wind is a N. wind, which comes from further N. than does the wind which arrives at the place itself as a N. wind; and the N.E. is therefore the heaviest, coldest and driest.

If, then, we suppose that after a northerly current gradually changing into an east wind, a southerly current sets in, we see the wind change from E. through S.E. to S., the barometer fall, and the air become warmer and moister. The longer this south current lasts, the greater, therefore, is the distance from which the wind comes, and the more the S. wind will change through S.W. to W.; for a S.W. wind is no other than a S. wind which has its origin further to the S. than a wind which arrives as a south wind. The winds between S. and W. are therefore south winds. If the cause which produced them continues, the south wind, which has become a west one, will keep back the direct air from the south; and there will be often repeated unsteady changes between S. and W. with thick weather. If at last the polar current presses forward again, we see the barometer rise with the wind passing through N.W. to north, while the temperature sinks and the clouds break.

This regular course prevails in the northern hemisphere, and the opposite in the southern one. We may therefore say that, on the average, the wind, from these causes, turns in both hemispheres with the sun, and that therefore the seaman's adage truly says—

> " When the wind veers against the sun,
>   Trust it not, for back it will run."

It follows, then, in regard to tempestuous movements of the atmosphere, that storms are either " gales or hurricanes." Both cause the wind-vane to turn round, but in the " gales " it turns only with the sun; in the rotatory storms or hurricanes it turns *with the sun on one side of the storm,* and *against the sun on the opposite side.* If a ship in the northern hemisphere is on the east side of a hurricane or cyclone advancing towards the north, the navigator cannot tell simply from the way in which the wind turns whether he is in a cyclone or in a gale; but if he is on the

west side he can tell, as he may generally infer, when a change of wind takes place against the sun and the wind blows strong, that the ship is in a cyclone.   It is not difficult to give for both hemispheres independent rules for judging on which side of the cyclone a ship is.   I have given such rules in detail in special memoirs, but I should make this letter too long if I were to enter on them, and they are also somewhat foreign to its immediate purpose.

<div style="text-align:center">Believe me, dear Sir,</div>

<div style="text-align:center">Sincerely yours,</div>

<div style="text-align:right">H. W. Dove.</div>

To Captain Washington, R.N., F.R.S.,
  Admiralty.

LX. *On the May Hill Sandstone, and the Palæozoic System of England. By the* Rev. Prof. Adam Sedgwick, *F.R.S., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HEREWITH I send what might, without impropriety, be called a continuation of the paper you have done me the favour to publish in the October and November Numbers of your Journal.   The task undertaken by Professor M'Coy and myself in 1853 was left incomplete; but we have this autumn taken it up where it had been abandoned, and completed our examination of various critical sections, at the junction of the Cambrian and Silurian rocks, which we had not been able to visit during the preceding year. Is there in South Wales any " Middle Silurian " group in which the characteristic Silurian and Cambrian types are so mixed and confounded as to be inseparable ?   In North Wales and Siluria we found no such group.   Wherever it had been erroneously laid down as one group we found it separable into two distinct stages—the upper of which contained a characteristic Silurian group of fossils—and the lower, an equally characteristic Cambrian group. But I was informed that near Builth, in some of the eastern hills of Radnorshire, and in several sections near Llandovery, the Government Surveyors had found the very mixture of older and newer types which we had sought for in vain during our short excursions in 1852 and 1853. To the places thus indicated (taking the Presteign sections on our way) we first bent our steps, and the results of our examination will be given in the early part of this communication.   They are in perfect agreement with what we had before seen in North Wales and Siluria.   There is, we believe, no " Middle Silurian group " like that laid down in the Government Survey—there is no confusion of organic types—the May Hill group (though in a degenerate and disconnected form) does exist, in the county here

alluded to, as a distinct formation—separable from the so-called " Lower Silurian " rocks, and constituting a physical and palæ-ontological base to the true " Silurian System :" and lastly, that " System," when reduced to its true base, is, we believe, either in actual position, or in palæontological succession, discordant to the Cambrian rocks on which it rests.

If these conclusions be true, there is an end of any legitimate dispute on nomenclature; for we have no example in English geology of two great formations which are, as a general rule, unconformable in their position, yet at the same time belong to a common series, and pass under a common name.

Having thus completed our observations on the groups connected with the May Hill sandstone, we next examined the sections through the Llandeilo group in the valley of the Towy. The results of this examination, and their bearing on the questions of palæozoic classification and nomenclature, will form the concluding part of the paper, which I now respectfully offer for insertion in the Philosophical Magazine and Journal.

<div align="center">I have the honour to be, Gentlemen,</div>

<div align="right">Your faithful Servant,</div>

<div align="right">A. SEDGWICK.</div>

Cambridge, October 31, 1854.

I have thought it best to retain the title adopted in my former communication, published in the October and November Numbers of this Journal. When the substance of this paper was read before the British Association (during September last) at Liverpool, the title was somewhat modified, for the express purpose of bringing the communication into a direct comparison with one of the preceding year, read before the British Association assembled at Hull, of which an abstract has been published in their volume for 1853.

The above letter to the Editors is perhaps à sufficient introduction to the new facts about to be described; but for the sake of any reader who is not familiar with the questions in debate, I think it best to give, in the first place, a short summary of the facts brought forward in my previous paper, and of the conclusions drawn from them. My special object was to prove—(1st) that the May Hill sandstone is the true and only base of the Silurian series (or "system"); (2ndly) that the Silurian series, when thus defined, was, as a general rule, either obviously unconformable to the Cambrian series, or overlapped its beds in such a manner as absolutely to conceal the true sequence of the deposits, as we pass from one series to the other. In confirmation of these conclusions (and in addition to the sections brought

before the Geological Society of London, Nov. 3, 1852) the following sections were discussed :—

The *first section* was from the Cambrian and Silurian rocks on the line of the Holyhead road, west of the Berwyns, and was as follows (*supra,* fig. 1. p. 306) :—

5. Carboniferous series.
4. Devonian series, very degenerate, and unconformable to the Denbigh flag.
3. Denbigh flag, &c. (Wenlock and Ludlow).
2. May Hill sandstone, of great thickness, and ending in a conglomerate. *Unconformable* to the Cambrian rocks.
1. Cambrian rocks of the Bala group.

The *second section* was taken from the sequence north and south of the valley of the Dee, near Llangollen (*supra,* fig. 3. p. 306).

5. Carboniferous rocks.
4. Denbigh flag. (No. 3 of fig. 3. p. 306.)
3. Upper Bala group, with two bands of limestone.
2. Alternations of slate and porphyry, trap-shale (*schaalstein*), &c., with many Cambrian fossils.
1. Beds of dark shale, grits, flags, &c., in which fossils gradually disappear.

Here there is no apparent unconformity, and the stage No. 3 might be almost said to pass into the stage No. 4; but the absence of the May Hill sandstone and conglomerate of the previous section proves that No. 4 has been brought over No. 3 by a great overlap, and that the section is therefore imperfect.

The *third section* was from Mathyrafal near Meifod (*supra,* p. 306. fig. 2).

4. Wenlock shale, &c. (No. 3 of fig. 2. p. 306.)
3. Beds of shale with many fossils (a good Cambrian stage).
2. Beds of limestone, coarse calcareous conglomerate, &c.
1. Fossiliferous series of the Meifod Hills, &c.

Here Nos. 1, 2 and 3 represent a peculiar development of the Upper Bala groups, and are collectively of great thickness.

Between Nos. 3 and 4 there is an obvious discordancy of position, and the May Hill series is wanting. It is probably overlapped by, and buried under, the Wenlock shale; and between Nos. 3 and 4 there was, so far as we saw, no interchange of characteristic Cambrian and Silurian species *.

The 4th, 5th and 6th sections from Norbury, the banks of the Onny and Shineton (where the Caradoc sandstone approaches

* I may remark, as I have done in a previous paper, that all the beds below the May Hill sandstone are here called Cambrian, whatever may have been their supposed sectional place in the "Silurian System."

the banks of the Severn), are described in the Report of the British Association (Transactions of the Sections, pp. 58 and 59. 1853). The general results derived from these sections are as follows (*supra*, p. 313. fig. 4) :—

Fourth section.

7. Carboniferous series.
6. Devonian series.
5. Ludlow group, in three stages.
4. Wenlock group, in four stages. $\begin{cases} d. \text{ Wenlock limestone.} \\ c. \text{ Wenlock shale.} \\ b. \text{ Lower Wenlock or Woolhope limestone.} \\ a. \text{ May Hill sandstone and Norbury limestone.} \end{cases}$
3. Caradoc sandstone of Horderley, &c.
2. Shales, &c. of Shineton $\begin{cases} \text{with } \textit{Agnostus pisiformis, Olenus?, Asaphus?,} \\ \textit{Citheropsis Aldensis, Siphonotreta micula,} \&c. \end{cases}$
1. Longmynd slates.

In this section Nos. 2 and 3 are obviously unconformable to the old Longmynd slates. But the base of No. 4 is also (though not so obviously) unconformable to No. 3. Hence this 4th section is not a continuous and complete, but is a broken and incomplete, section. It has not therefore been correctly assumed as a typical section; nor can it, under the name Caradoc sandstone, be made the foundation of a correct nomenclature *.

So far I have been dealing, in the way of recapitulation, with the evidence of my preceding paper : and bearing on exactly the same questions, I now proceed to comment on some additional evidence which fell under the notice of Professor M'Coy and myself during our short recent tour made in a portion of South Wales.

I. *New Radnor and Presteign Sections.*

The accompanying sketch does not profess to exhibit an accuracy of outline or exact proportion among the beds; but it does, I

Fig. 1.

Old Radnor.

1. Trap.
2. Limestone dipping N.W.
3. Wenlock shale.

2′. Limestone dipping S.E.
3. May Hill sandstone.
2″. Reappearance of the limestone near Presteign.
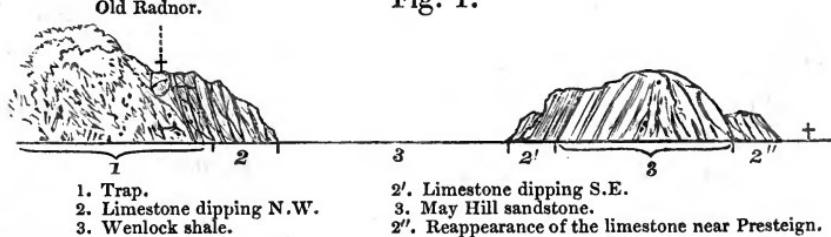
believe, exhibit the right order of the several groups, and therefore may help to illustrate the following remarks. The lower parts of the valley extending from Presteign to Old Radnor are

* (See fig. 4. p. 313. *supra*.) The black line, between Nos. 2 and 3 of this section, is drawn inaccurately. It ought to be parallel to the dividing lines of No. 3; for 2 and 3 are perfectly conformable and in an unbroken sequence.

probably filled with Wenlock shale, and near the latter place some large protruding bosses of igneous rock have elevated a fine mass of limestone, which dips in a north-westerly direction towards the valley. On the opposite side of the valley, and about two miles from Presteign, rises another and a similar mass of limestone, dipping also towards the valley, in a south-easterly direction and at a great angle. From beneath this second mass of limestone rise some thin beds of arenaceous and calcareous shale, succeeded by beds of yellowish sandstone of considerable thickness, which are thrown into a saddle, and form a picturesque ridge. Near the summit of the ridge the sandstone becomes more coarse, and passes into a conglomerate very like that near the top of the well-known beds of May Hill. As we descend from the ridge towards Presteign the dip becomes reversed, and we again meet with the limestone, which (though broken and interrupted) seems to have wrapped round the protruding ridge, so as to dip, wherever it appears, from the sandstone.

If the shale of the valley be Wenlock shale, the limestone must be in the place of the limestone of Woolhope ; and its fossils are unquestionably those of the Wenlock group. This conclusion seems now to be admitted, though at one time controverted. But what is the sandstone forming the ridge under the limestone ? The beds under the limestone, to a considerable depth, contain many fossils ; while the lower and coarser beds which break out near the summit of the ridge appear (like those near the top of May Hill) to be almost without traces of organic life. We collected during an evening walk the following species, which have been carefully determined by Professor M'Coy.

Sandstone Beds.

Petraia (unnamed species, same as at May Hill).
—— *subduplicata*.
—— *uniserialis*.
Hemithyris hemisphærica (abundant).
—— *diodonta*.
—— *didyma* (as at Aymestry).

Strophomena simulans (very abundant).
Pentamerus oblongus.
—— *lens*.
Orthis elegantula.
Spirifera crispa.
Modiolopsis ?
Loxonema elegans ?
Murchisonia ?

Old Radnor and Presteign Limestone.

Stenopora fibrosa (in very great abundance).
Favosites alveolaris.

Spirigerina reticularis.
Hemithyris bidentata.
Orthis testudinaria.

So far as the above two lists extend, there is in them no species not well known either at May Hill, or the corresponding beds at Malvern, or in the undoubted Wenlock and Ludlow

rocks of other localities. One of the species (*H. didyma*) had not been found lower than the Aymestry limestone; and *Spirifera crispa* not lower than the Wenlock limestone. A few of the species, as is well known, have a much wider range.

It follows from the above facts, and especially from the list of fossils, that the sandstone of Presteign is a true May Hill sandstone: but it does not exhibit its relations to any inferior group; neither does it exhibit any mixture of the characteristic Cambrian with the characteristic Silurian types.

II. *Sections through the Llandeilo Flag and the Trappean Hills north of Builth.* (Fig. 2.)

The rocks here noticed are too well known to need any elaborate discussion, as they are excellently described in the ' Silurian System.' They break out immediately on the west side of the great terrace which runs into Mynydd Epynt, and is composed of the Upper Silurian rocks (Wenlock and Ludlow). These Builth rocks do not, however, dip *under* the " Upper Silurian " rocks, but *from them* towards the west—where they are overlaid by the Norbury limestone and the Wenlock shale. The shale extends several miles further west through a comparatively low country; and beyond it rise the higher hills of Radnorshire, which in the ' Silurian System ' are coloured as Cambrian; and have always been considered by myself as a part of the Upper Cambrian series. But the author of the " System," in adopting the nomenclature and colours of his map and sections, took it for granted that the Builth flags were geologically superior to these higher hills of Radnorshire; and if the assumption be incorrect I am not in any way responsible for the mistake. For I never examined, or professed to have examined, the physical base of the so-called " Lower Silurian " rocks of South Wales; nor did I ever see a single section of them between the summers of 1834 and 1846. As a matter of fact, the Builth rocks show no unequivocal relations either to the Cambrian or Silurian series; being surrounded by a sea of Wenlock shale, which abuts against their lower beds on the eastern side and overlaps their upper beds on the western.

The accompanying section may be shortly described as follows :—

Fig. 2. *Section about two miles N. of Builth, nearly E. and W.*

W. Pen Cerrig. E.

5 4 3 2 1 5

1. Trap.
2. Alternations of recomposed trap and shale.
3. Dark flagstone ending with a bed of recomposed trap.
4. Norbury limestone, &c.
5. Wenlock shale, &c.
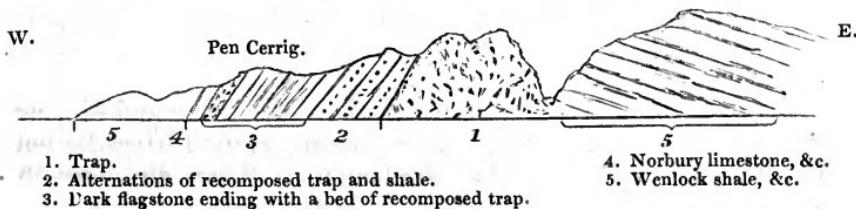
1. Immediately on the west side of the Silurian hills is the rugged trappean ridge which forms the base of the section. Some parts of it are highly cellular and amygdaloidal; and if it were originally submarine, it must have been formed at least in a very shallow sea.

2. Next follows a remarkable series of beds, which towards the north become greatly expanded, and which on the sectional line are of considerable thickness. They are made up of a kind of trappean breccia, and of a fine granular recomposed trap, in some places forming a good building-stone. And these masses alternate with some beds (especially in the more northern parts of the range) to which the name *ash* might, I think, be fairly given—though I greatly limit the meaning of the word *ash*, as used in the Government Survey. In its place I formerly used the term plutonic silt, and afterwards the German term *schaalstein*; and in the same sense I now generally use the words *trap-shale*; defining thereby a vast series of slaty recomposed rocks derived chiefly from submarine eruptions, which therefore never existed as a *true ash*. Alternating with such masses are also found several bands of a true shale and flagstone, evidently forming a part of the regular aqueous flagstone of Builth. All the above beds occasionally contain fossils, among which I may mention the *Asaphus tyrannus,* of which a noble specimen was found in one of the plutonic grits.

3. Over the above, on the sectional line, is a considerable series of beds of dark shale and flagstone in which the igneous recomposed rocks almost entirely disappear. It was from this series (about a quarter of a mile east of Pen Cerrig) that in 1846 I collected, along with my fellow-labourer John Ruthven, a fine suite of fossils, some species of which were new. They are included in the list of 24 species of Builth fossils given by Professor M'Coy (Camb. Palæozoic Fossils, p. 354). The locality is stated to be three miles north of Builth, but the true distance is not much above two miles.

The series ends just below Pen Cerrig House, where it is overlaid (as seen near a pond and small water-course) by a well-defined bed of recomposed trappean rock, also containing traces of fossils.

4. Next follow calcareous and sandy beds, apparently in perfect parallelism to the former. They are not more than 8 or 10 feet in thickness, and the more calcareous portions are good types of the Norbury limestone, and contain innumerable casts of the characteristic Pentamerus.

5. Over the preceding, and with the same strike and dip, are beds of shale and earthy calcareous flagstone, with irregular lenticular concretions parallel to the bedding. These dip down the water-course and form the highest beds visible in the section.

The beds (in a quarry under Pen Cerrig House) which are *immediately under* the recomposed trappean bed are made up of an indurated Builth flag like that of the well-known Well field quarries, and contain *Ogygia Buchi* and *Diplograpsus pristis* in considerable abundance.

The shales (No. 5) over the Pentamerus beds contain *Graptolites Ludensis*, an unnamed Denbigh flag species of *Atrypa*, and *Orthoceras* (?) ; and we found in them no other fossil.

To exhibit the evidence in one point of view, I here subjoin a list of fossils we collected from the beds in the above section, including the flags of Wellfield quarries, which are a little further south, but on the line of No. 3, and not far from its top.

1st. From the Llanelwedd quarries of plutonic grit, &c. (the building-stone of Builth), near the turnpike road.

Pyritonema fasciculus, the same as at Tre Cib near Llandeilo.
> This species has sometimes been mistaken for an *Ichthyodorulite.*

Orthis avellana.	*Leptæna sericea.*
O. calligramma.	*Pterinea.*

All of which are Cambrian species, *i. e.* are not found, so far as we have seen, in any May Hill or Upper Silurian rocks.

2nd. From a quarry east of Pen Cerrig. (No. 3 of the figure.)

Diplograpsus pristis.	*Ogygia Buchi* (abundant).
Ampyx nudus (very abundant).	*Cytheropsis Aldensis.*

All the above are Cambrian; and a much larger number of Cambrian species from the same quarry is given in the published list of the Cambridge Palæozoic Fossils (p. 354).

3rd. From a quarry west of Wellfield House.

Diplograpsus pristis.	*Ogygia Buchi* (abundant).
Ampyx nudus.	*Siphonotretra micula* (in great
Agnostus Maccoyi (Salter).	abundance).

All the above are exclusively Cambrian. In the Wellfield grounds are some beds very much resembling the shales of Shineton near Wenlock. Should the *Olenus* be found in them, it will appear among its former associates and among rocks of the same age with those of Hollybush and Shineton.

4th. From the quarry below Pen Cerrig House.

1. Lower indurated shale under the trap. { *Diplograpsus pristis,* *Ogygia Buchi,* } exclusively Cambrian.

2. Bands of Norbury limestone, &c. above the trap. { *Petraia* (as at May Hill; an unnamed species). *Ptylodictya lanceolata* (Wenlock). *Pentamerus oblongus* (in very great abundance). *Orthis pecten.* No exclusively Cambrian species, and one exclusively Wenlock, hitherto.

3. Upper shale. { *Graptolites Ludensis.* *Atrypa* (an unnamed Denbigh flag species). *Orthoceras* (?) (Wenlock shale).

From the above statements the following conclusions seem naturally to follow :—

1st. The Pentamerus beds (or Norbury limestone, No. 4) must be arranged with the shales (No. 5. fig. 2), as their fossils belong to a group which is entirely distinct from that of the Builth flag. The case is exactly like that at Norbury and in the neighbourhood of Wenlock, where the same beds were cut off from the Caradoc sandstone, and arranged by Professor M'Coy and myself as the base of the upper or true Silurian series. Here (just as in Bohemia) there is, after the intervention of a bed of trap, an *immediate* change in the fossil fauna—the upper being Silurian, while the lower is Cambrian : and a similar remark applies, we believe, to one of Professor Phillips's Malvern sections, which is based on the Hollybush shales.

2ndly. Spite of the almost mathematical parallelism in the bedding of the preceding groups, there can be little doubt but a great geological interval exists between the true Builth flags (No. 3) and the overlying beds (Nos. 4 and 5). The section is not perfect and continuous, and cannot (on that account) give us a guidance to a true nomenclature of the groups. The case is analogous to that already quoted from the section south of Llangollen (*supra*, fig. 3. p. 306).

III. *Conglomerates, slates and sandstones of Dol Fan, &c.*

Under this title is included a large and ill-defined group of slates and sandstones, sometimes passing into a coarse conglomerate. This group, commencing near the southern end of Radnor Forest, ranges through Dol Fan towards the south-west ; generally contorted and forming saddles with sides of high inclination ; the conglomerates not continuous, but breaking off and reappearing ; first forming a part of the mountain barrier to the comparatively low country of Wenlock shale which runs to the SW. of Builth ; then thrown further back among the western mountains ; and at length forming a kind of high rugged plateau near the water-shed of the Cothi and the Towy. That this singular and ill-defined group has a high geological place among the undulating masses between Teifi and the Towy can admit of no doubt ; and I think it certain (as I have done since 1832) that these undulating masses (as well as many others of a very different type, further west towards Cardigan Bay) are superior to the Bala limestone.

Of what date then are the Dol Fan conglomerates and the associated beds of grit and slate ? The answer can only be supplied by the fossils. In my old collection, made in 1846, I have but one species from the neighbourhood of Dol Fan, viz. *Euomphalus tricinctus.*

During the year 1846 I made, however, another traverse

through the conglomerates, which are finely exhibited between Llandovery and Pumsant; and at Bwlch Trebennau I found the following fossils, which were determined by Mr. Salter :—

Encrinite stems (abundant).	*Orthis elegantula* (abundant).
Favosites and other ill-preserved corals.	*Atrypa crassa.*
Turbinolopsis (a large species).	*Calymene,* small fragment not specifically determinable.
Leptæna sericea (abundant).	

The parcel containing this list never reached the Cambridge Museum, and Bwlch Trebennau does not, therefore, appear in M'Coy's alphabetical list of "Lower Palæozoic Localities *." But I lately revisited the same spot, along with him; and we procured from it the two following species, *Euomphalus tricinctus* and *E. triporcatus.* Both species are true Cambrian types; and the former helps us to connect the conglomerates of Trebennau with those of Dol Fan. Taken collectively, the above list, however imperfect, seems clearly to indicate a group *below* the May Hill sandstone, and is therefore Cambrian.

IV. Intimately associated with the above conglomerates, and, I believe, on a lower geological stage, is a very great development of rather earthy slates, and of arenaceous flagstones, sometimes coarse, and almost deserving the name of conglomerates, in which case they are often ferruginous. The shaly slates of this group are well seen in the high hills which form the watershed between Builth and Llandovery, both on the old road and on the newer road by Llanwrtyd and the Sugarloaf. Beds of the same general age range far up the hills on both sides of the Towy above Llangadoc, and here and there show somewhat rugged crests of hard, brown sandstone or conglomerate.

This group is in some places extremely fossiliferous, and may be well seen in an excursion northward from Llandeilo to Taliaris, and round the hills near Garn Fawr. This excursion was made by Mr. Salter and myself in 1846, and we obtained fossils from the following localities, viz. below Taliaris, Park Lodge, Garn Fawr, Cil-y-waun, and other places on our track. I have in my note-book the lists made out by Mr. Salter, with which I will not trouble the reader, as the fossils may be far better studied in the Museum of Practical Geology than in the Cambridge collection. But this I may state, on the authority of Professor M'Coy, that so far as my lists and species go, they form a very characteristic upper Bala group, and contain not so much as one species which can be regarded as characteristic of a May Hill or a Wenlock group.

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* At the same time was lost a small but valuable parcel, containing the fossils from the neighbourhood of Devil's Bridge.

The above great and ill-defined group seems to range through the water-shed last mentioned, and so to pass under the Wenlock escarpment. On the southern side of the valley between Llandovery and Llangadoc the same group is exposed, and from various quarries (*e. g.* Pen-y-lan, Goleugod, Cefn Rhyddan) I obtained the species described by Professor M'Coy in his List of the Cambridge Palæozoic Fossils. As among them were some species which rather seemed to indicate a May Hill group, it became important to re-examine some of the localities, which I will notice in the order in which we visited them.

1st. *Penlan* (or *Pen-y-lan*), a little more than a mile E.S.E. of Llandovery.

The quarry with the fossils is near the crest of the hill, and contains some beds of sandstone, alternating with a decomposing flagstone and indurated shale. It was formerly used for building, but is now deserted. The beds dip *at a great angle* about N.W. Some of the beds of the ridge, which ranges to the N.E., are nearly perpendicular. In a new quarry, nearly a mile towards the N.E., and at a lower level, we found the beds dipping about S.E. at an angle of 70°. We believe it almost impossible to determine, by the evidence of sections, what exact relation the Penlan beds bear to the rocks in the immediate neighbourhood; but among the ruins of the deserted quarry we collected the following species:—

Halysites catenulatus.	*Stenopora fibrosa.*
Petraia uniserialis.	*Pentamerus globosus.*
—— *bina.*	—— *oblongus* (very abun-
Palæopora megastoma.	dant).

This list does not contain any species which has hitherto been regarded as exclusively characteristic of Cambrian rocks, and it gives us a true and characteristic May Hill group of fossils.

2nd. *Two quarries from the Goleugod ridge,* near the road on its north side, and a third quarry on the cross road to Myddfai gave the following results:—

1st quarry—

Halysites catenulatus.	*Spirifer percrassus* (very abun-
Favosites alveolaris.	dant).
—— *multipora.*	*Pentamerus undatus.*
Ceraurus Williamsi.	*Orthis calligramma.*
	Lituites cornu-arietis.

The corals and the Pentamerus are well-known common types, the others are exclusively Cambrian; therefore the group is Cambrian, as it does not give one exclusively Silurian type.

2nd quarry—

Petraia uniserialis.	*Orthis parva.*
Ceraurus Williamsi.	—— *elegantula* (var *α*).
Orthis protensa (in great abundance).	—— *turgida.*
	Leptæna sericea.
—— *testudinaria.*	—— *quinquecostata.*
—— *flabellulum.*	

This is a very characteristic Cambrian group, containing only two species, which are common to the May Hill beds.

The *third quarry* we had not time to examine in detail, but it contained *Leptæna sericea* abundantly (a good Cambrian species, never found by us in the May Hill or Wenlock groups).

Here (as in the former case) we are not able to define the place of the Goleugod ridge by help of the sections. The country is broken and faulted, and all the beds are highly inclined; but there cannot be a doubt that the ridge is, according to our nomenclature, Upper Cambrian—a part of the upper Bala group. It contains no quarry with a group of fossils like that of Penlan. In the gravel over the rocks of the second quarry we, however, found water-worn masses with casts of *Pentamerus oblongus,* which seemed to show that other May Hill beds were not far off.

V. *Quarries on the Hill of Mandinam.*

The accompanying section, without pretending to any accuracy of detail, conveys, I believe, a correct idea of the relative

Fig. 3. *Mandinam Section nearly N. and S.*

1. Hills of Wenlock shale and Old Red Sandstone. 3. Beds with Cambrian fossils.
2. Confused and mineralized beds. 4. May Hill group.

position of the several groups. To the south are the escarpments of the Old Red Sandstone, and of the Wenlock and Ludlow groups. Then follows a valley in which the beds are dislocated, mineralized, and penetrated by lead veins, which we had not time to examine, as they are considerably to the east of the line of section. Lastly follows the Mandinam ridge, which is very fossiliferous.

1st. Rocks near the top of the hill.

Encrinurus punctatus.	*Leptæna sericea* (abundant).
Orthis calligramma.	—— *quinquecostata.*
—— *testudinaria.*	

Two of the above (*E. punctatus* and *O. testudinaria*) are sometimes found in Silurian rocks, the others are exclusively Cambrian.

2nd. North side of the hill, from an old quarry, and thence down to the level of the river.

Stenopora fibrosa.	*Pentamerus oblongus* (abundant).
Petraia uniserialis.	—— *linguiferus* (including
Spirigerina reticularis.	*undatus*).
Leptagonia depressa.	*Turritella cancellata* (?).

Here we have not one exclusively Cambrian species, but several which are common to Cambrian and Silurian rocks. Like that of Penlan, it is a good May Hill fossil group.

Such were the results of a single day's examination of the very broken and difficult country to the south of Llandovery. It would, perhaps, have been impossible to arrange its beds in any approximate geological order had there been no clearer evidence in any other typical country. There are, however, in this country two very distinct fossil groups,—a May Hill group, and an upper Bala or Cambrian group. These two groups are in the quarries exhibited apart. They do not appear to alternate or interchange places. They have several species in common, but there is no confusion among the typical species. Thus, no truly characteristic Cambrian species was found in the upper or May Hill beds; and, in like manner, no truly characteristic Upper Silurian or May Hill species was found in the Cambrian beds. Such were the facts observed, whatever be the consequences to which they may lead us. The facts are stated fairly and without reserve.

VI. *Section of Castell Craig Gwyddon, &c.*

For purpose of illustration, I here exhibit a reduced sketch of a small part of two beautiful sections published by the Government

Fig. 4.

N.W.   Castell Craig Gwyddon.   S.E.

Noedd Grug.

6   5   4   3   2   1
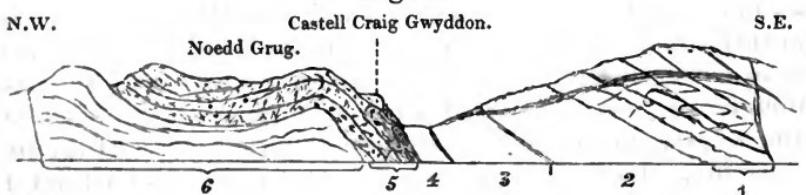
1. Old Red Sandstone.
2 and 3. Ludlow and Wenlock.
4. Probably Wenlock.

5. May Hill beds of Castell Craig Gwyddon, probably continued through Noedd Grug.
6. Cambrian group, geologically far above the Llandeilo flag in the valley of the Towy.

Survey and the author of the 'Silurian System.' Of the general

accuracy of these sections I have no doubt; though I may remark, that the cleavage planes of Noedd Grug are not given in the section of the Survey quite correctly, for they deviate with the flexures of the beds; so that in one place, where the beds are highly inclined, the cleavage planes (if I may trust my remembrance of the phænomena after a lapse of twenty years) become nearly horizontal. It was in the hope of solving this peculiarity of structure that in 1834 I accompanied my friend Sir R. I. Murchison over Noedd Grug; and it was the *only instance in this part of Wales* in which I went with him towards the beds which he then regarded as the geographical boundary of his system. He placed the boundary a little beyond the northern limit of the ridge.

I lament that an attack of indisposition (the only interruption during our short tour) prevented me from accompanying my friend Prof. M'Coy over the ridge of Noedd Grug; and he was so entirely unacquainted with the fossil localities that he did not wish to make the traverse by himself. Our observations were therefore confined to the south end of the ridge (Castell Craig Gwyddon), and to the shale beds immediately overlying it.

1st. Respecting the Old Red Sandstone, and the greater part of the great Silurian group (Wenlock, &c.), there can be no dispute. 2nd. As to the stage of shale and earthy slates (No. 4 of the figure) which come next in order, we sought diligently for its fossils, but we did not find so much as one; and from its mineralogical type, we did not see how it could either be classed with, or separated from, the Wenlock shale. 3rd. The hard, and nearly vertical, beds of gray sandstone which rise from beneath the shale are too well known to need detailed description. In places they become of coarse structure, but they do not, so far as I know, ever pass into a coarse conglomerate, like that of Dol Fan or Bwlch Trebennau.

The upper beds of the great precipice contain (as will be shown) a fine May Hill series of fossils. Hence it seems natural to conclude, that the shales immediately overlying these beds are a true part of the Wenlock shale; an opinion I should hold, unless the contrary could be proved by fossil evidence, which probably will not be done. Whether the May Hill group of fossils extends through the whole of the contorted sandstone group of Noedd Grug, or is confined to its upper portion, we were unfortunately unable to determine, for the reason above given. But I think I may affirm, from the remembrance of bygone years, that to the north of Noedd Grug, and near Cefn-y-garreg, true Cambrian fossils are met with in abundance. Not, however, to detain the reader with conjectures, I subjoin the list of fossils collected at Castell Craig Gwyddon.

Stenopora fibrosa.
Halysites catenulatus.
Petraia (an unnamed species, same as at May Hill).
—— *bina.*
—— *uniserialis.*
—— *subduplicata.*
—— *rugosa* (?).

Palæopora interstincta.
Pentamerus linguiferus (+ *undatus*).
—— *globosus.*
—— *oblongus.*
Hemithyris hemisphærica.
Leptæna transversalis.
Spirigerina reticularis.

Of characteristic Cambrian species (*i. e.* of species which have not been found among any admitted Upper Silurian rocks), *not one* occurs in the above list.   But *all* the above species are found in the Wenlock or May Hill beds.   And I may remark that here (as at Norbury), nearly on the line of demarcation between Cambria and Siluria, the common species seem to be crowded together, and we might say that the *old* characteristic types had disappeared, and that the *new* and characteristic Silurian types had not yet found time and place to develope themselves on the section.

On the joint evidence of this and the preceding section, we may therefore conclude that we have found a true May Hill group—a palæontological and physical base to the Silurian rocks, —that this base does not lose its character by a blending within it of the characteristic Cambrian and Silurian types, and hence that (spite of the enormous dislocations of the country) it does give us, though in a disjointed and fragmentary form, the elements for the construction of a good geological horizon.   I do not, however, believe that this horizon would have been discovered in the country above described, had not its place been *made out before*, by the incomparably clearer sections in North Wales and the bordering English counties.

I here conclude the details connected with the establishment of the May Hill sandstone as a group not to be confounded with, or united to, the Cambrian rocks.   It was deposited after, or during, great mechanical movements, and it seems exactly to mark the epoch when the characteristic types of an older fauna disappeared, and when the characteristic types of a newer fauna began.   If the facts above stated be not invalidated by opposing evidence (and of this I have little fear, as I have now examined the very places where it had been thought that there *was* some opposing evidence), the discussion of them is no idle battle of mere words, but involves fundamental principles essential to a right classification, and consequently a right nomenclature of our older Palæozoic series.   No classification, therefore, or nomenclature which has overlooked the place and office of the May Hill sandstone can rest on a basis that is secure, or have any claim for a permanent acceptance.

VII. *Llandeilo Flag of the Valley of the Towy, and its relations*
to the neighbouring groups.

I will not detain my readers by any elaborate details respecting
this remarkable group. It had long been known to fossil col-
lectors from its specimens of *Asaphus Buchi* and other fossils,
and for ages its limestone beds and flags had been quarried for
economical use, but the definition of its geological place was
first attempted by the author of the 'Silurian System.' It
contains many calcareous bands, as may be well seen in the
Silurian sections, and still better in the maps and sections of
the Government Survey. These limestone bands (some exhibited
as mere streaks of limestone, alternating with, and passing into
dark shale and flagstone, others as thick and solid masses) are
numerous. Within the group are also bands of a brown, arena-
ceous flagstone, and sometimes of very hard and coarse siliceous
grits passing into a conglomerate. So far as I know, its base is
not exposed in any section of the Towy, but its highest beds (as
I interpret the group) are on its north side composed of a dark-
coloured, pyritous, indurated shale, passing into a slate, which in
a few places (*e. g.* N.E. of Carmarthen, and at Pengoylan near
Llangadoc) has been quarried for use. Some of these slates,
not being affected by transverse cleavage, show the *Diplograpsus
pristis* in considerable abundance.

The group is in the form of a saddle, but these highest bands
are not so well exposed on its south side as they are on the north
side. They are represented partially, and may perhaps be here
and there overlapped and concealed by the Wenlock shale, which
(on the south side of the Towy from Llangadoc to Llanarthney)
seems to form the immediate southern boundary of the group.

Most of these peculiarities of structure are seen in the sections
published by the Government Survey, and in the original sec-
tions of the 'Silurian System;' and with no idea of blinking
the difficulties of the subject, but in the hope of making the
relations of the groups clear, I gave an ideal section (*supra*, fig. 6,
p. 314) to show the order of the deposits down the valley of the
Towy, through a space of nearly twenty miles below Llangadoc.

The disturbing forces that caused the complicated derange-
ment of the Llandeilo group affected also the beds much further
towards the north-east, but without bringing up the pommel of
the saddle; for above Llangadoc, the valley for several miles
must originally have been bridged over by the higher geological
groups, such as those above described, which ended, here and
there, with a kind of top dressing of the May Hill sandstone.

The slight ideal sketch (fig. 3, p. 314) above noticed was from
memory; but since my recent visit to the valley of the Towy, I

wish to expunge No. 3 (the Caradoc sandstone) from the section, and to place the Llandeilo flag immediately under the Wenlock shale.   The hypothetical interpolation of the Caradoc sandstone did not, however, at all affect the general inference I drew from the sketch.   It conveys, however, such an inadequate notion of the thickness of the nearly vertical beds which overlie the north side of the Llandeilo saddle, that I will endeavour to give a better ideal representation of it.

Fig. 5.

N. and S. section through the valley of the Towy below Llangadoc.

S.   Valley of the Towy.   N.

1   1a   2   3   4   5

1. Old Red Sandstone.
1a. Tilestone.
2. Wenlock and Ludlow.
3. Contorted Llandeilo saddle.

4. Highly inclined slate rocks, &c. of very great thickness.
5. Commencement of the great undulating groups.
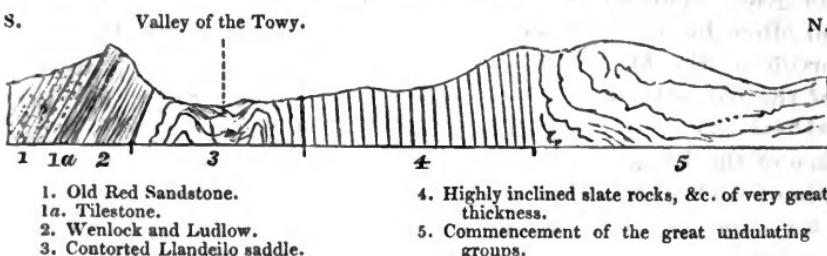
If the above sketch convey anything resembling a correct notion of the relations of the great mineral masses, it must inevitably follow that the Llandeilo flag is no true base to the overlying Silurian terrace, and that it has no immediate relation to the Wenlock shale except that of an accidental juxtaposition arising from an irregular overlap of the true Silurian groups; on the contrary, that it is deeply imbedded in the great Upper Cambrian group, which rolls, in many times repeated and irregular undulations, to the coast of Cardigan Bay; or, if it be crossed on a more northern line, which rolls in like undulations till it is lifted up and arrested near the south-eastern flank of Cader Idris. I call the group Upper Cambrian, because, however different in mineral type, it is the representative of the great Bala group (No. 3) of the Tabular View (*supra*, p. 362)*.

* I once, with a view of bringing the older rocks of Wales into comparison with those of Cumberland, limited the Upper Cambrian group to the rocks which commence with the Bala limestone and end with (what I now call) the May Hill sandstone.   The change I afterwards made (in drawing the Upper Cambrian base line on the S.E. flank of Cader Idris or the E. flank of great Arenig) involved no change of principle; it was a mere shifting of a line of demarcation among the subdivisions of an unbroken series.   Whether the calcareous beds of the Llandeilo group be, or be not, the exact equivalents of the Bala limestone I do not pretend to determine.

    The May Hill sandstone, which in the typical Silurian country is of great thickness and continuity, gradually dies away and disappears in the country described in this paper.   Hence it sometimes appears and sometimes disappears from the base of the overlying Silurian groups; and in the latter case, the Wenlock shale forms the base of the Silurian groups

If this be true, the ideal section (*supra,* fig. 5, p. 313) appealed to for years before and after the publication of the ' Silurian System,' as the basis of nomenclature for its lower groups, was absolutely without meaning. If the fundamental sections were wrong, the nomenclature could not be right. In the determination of the true Silurian groups, the author of the " System " did incomparable service to the cause of geology, and there is not a geologist in the old or new world who is not grateful for it. But in descending below the groups he had irrefragably established, he missed the key of his own position, and linked together groups which had no nearer relation than that of accidental, and often discordant, juxtaposition. When he passed below the horizon of the May Hill sandstone, he did the collector's work and the naturalist's work admirably well; but he failed in the real task-work of the geologist—in the definition of the true place of the Llandeilo group in the great Cambrian series; and without such a definition of the group, its nomenclature, as part of a system, is absolutely untenable and without meaning. And the same conclusion (since the elimination of the May Hill group) applies also to the Caradoc sandstone.

That the Llandeilo flag is a subordinate part, and not a high part, of the Upper Cambrian or Bala group is, I think, sectionally evident; but we cannot, by the help of the South-Welsh sections, eliminate its exact place, or its clear coordination with any ascertained stage of the Upper Cambrian group of North Wales, nor is it at all necessary that we should do so. The Llandeilo fossils are well known, especially from the excellent

which overlie the Cambrian rocks. That the Wenlock shale is in such cases unconformable to the Cambrian rocks is evident from the following facts, which would prove the point independently of the evidence supplied by the above section. To the south of Llandovery this shale ranges over rocks which are geologically higher than the Llandeilo flag; then for about a dozen miles it appears to range immediately over the Llandeilo flag; and afterwards, in its western prolongation, it again wraps over a group which is, I think, higher than that of Llandeilo.

It is not always an easy matter to determine whether two contiguous groups be conformable: and if (as in the case before us) the groups have been elevated and contorted by the disturbing forces of a newer epoch, it may become very difficult to make out the fact of discordancy between the groups. I believe that the preceding remark partly explains the great difficulty in making out the true relations of the Upper Cambrian rocks of South Wales. They were first exposed to those disturbing forces which produced the great north-eastern and south-western undulations of North Wales; and they were afterwards crushed, dislocated, and contorted by those great east and west disturbing forces which produced the actual configuration of the great South-Welsh coalfield. There can, I think, be no doubt that we owe the principal phænomena in the valley of the Towy to the action of the latter set of forces upon beds which had been dislocated at a prior period.

lists published in the 'Silurian System,' and in the descriptive lists of the Cambridge Palæozoic fossils by M'Coy.   We added, during our late hasty visit, only one new species, and it would be idle for me to give many extracts from my note-book; but I will make one exception in favour of the quarries of Bird's Hill near Llandeilo, where there are several beds of limestone which dip N.W., and are on the northern side of the Llandeilo saddle.

In those quarries are three or four bands of limestone marked by lines of old and generally deserted works, and the calcareous portions are more like the Coniston limestone and its associated calcareous slates, than any other portions of the Llandeilo group in the neighbourhood.   From these beds we obtained the following species :—

1st quarry, the upper limestone band.

Asaphus tyrannus.	*Orthis expansa.*
Orthis retrosistria.	*Trinucleus Caractaci.*

All exclusively Cambrian.

2nd quarry, from the third band of limestone to the S.E. of the former.

Stenopora fibrosa.	*Spirifera biforata*, var. of *fos-*
Ptylodictya, n. s.	*sicostata* (abundant).
Orthis porcata.	*Leptagonia depressa.*
—— *avellana.*	*Leptæna sericea.*
—— *elegantula* (var. α).	—— *uralensis* (?).
—— *parva.*	*Bellerophon ornatus* (?).
—— *testudinaria.*	*Illænus.*
—— *callactis* (Dal.).	*Ceraurus clavifrons.*

The species (like all the rest in this paper) are determined by Professor M'Coy; and he remarked, on the spot, that "we have in this second quarry what I should call a good Bala list;" and it perhaps deserves remark, that the list is wanting in some of the very characteristic species of the Llandeilo flag, such as *Asaphus tyrannus* and *Ogygia Buchi*, &c., which brings it still closer to the fossil lists of Bala and Coniston.   But it admits of no doubt, that all the Bird's Hill beds are an integral part of the Llandeilo group.   Hence, without pretending to fix exactly the place of this group, we have good palæontological, as well as sectional evidence, to prove that it is deep packed in the Upper Cambrian (or Bala) group, including under that term all the rocks of the Cambrian series which are above the Arenig porphyries and under the May Hill grits.

On a review of all the facts above described, I can now affirm, on fuller evidence than was given in my preceding paper (which was based on the sections observed in 1853), that the scheme of

classification, and consequently of nomenclature, given in the Tabular View (*supra*, p. 362) is true for the whole Cambrian series,—that it is the only approach to a good analysis of this series which has yet been given—that it was worked out correctly by my own labours among the older rocks of Wales, and has the claim of priority—that it is geographically appropriate; that it is physically true, because it makes a break between the Cambrian and the overlying Silurian series where nature herself had made it—and lastly, that it is palæontologically true, inasmuch as that break is followed by an immediate and great change of the fossil species. It is true, exactly for the same reason that Smith's original classification and nomenclature (however uncouth and harsh sounding) of the Oolitic series was true—because founded on true sections backed by lists of fossils. It is true, for the same reason that Mr. Prestwich's analysis of the deposits above our chalk is true. Lastly, it is true, for the same reason that the classification and nomenclature of the "Upper Silurian" rocks is true. Nor is there, in the past history of British geology, any example of a single group, or series of groups, which has stood its ground and been established, except on principles like those which I have, I believe, correctly followed, and by the test of which I am willing that the scheme of classification and nomenclature, above given in the Tabular View, should stand or fall. That it will stand I have not the shadow of a doubt, so long as we adhere to our geographical nomenclature. That it may be superseded, hereafter, by some more general and scientific classification which may embrace the old and new world, and in which all merely geographical and local names will disappear, is quite possible, and I trust probable : but such a speculation does not come within the limited views of this paper.

But now arises another question, Is there any other scheme of classification and nomenclature which has a higher claim to our acceptance? The two, and only, "Lower Silurian" groups were based on the evidence of a section above given (*supra*, fig. 5. p. 313). When properly filled out and subdivided it becomes the very section exhibited again and again before the Geological Society of London during five or six years preceding the publication of the 'Silurian System,' and in illustration of the Author's scheme. It is virtually the same with the illustrative section engraven on the original Silurian map. By myself it was accepted for eight years (during which I never revisited the Silurian country) with almost implicit faith and on the sole authority of the Author : and I might almost say the same for all geologists who had studied the palæozoic rocks.

But would geologists have accepted the "Lower Silurian" classification and nomenclature had they known that the phy-

sical, or sectional, evidence on which it was based had from the first been positively misunderstood ? I cannot answer such a question in the affirmative.

When the progress of information proved that the Caradoc group had in many places been misinterpreted, and that the Llandeilo group had been placed at the base of the " System " by a great misconception of the Llandeilo section, one might have looked for *some modification* of the nomenclature. On the long run it would have been wise for the Author of the " System " to have changed his nomenclature : but for present advocacy it would have been unwise ; for those who have accepted and used a nomenclature never willingly change it. And in geology, as in every thing else, to prevent the inconvenience of change, many men will abide in a false position and sometimes even acquiesce in a gross absurdity. A bad rule, however, for scientific progress.

The Silurian nomenclature professed to be at once geographical and sectional : and how was it to preserve its character with the destruction of its base and its sudden extension over Cambria ? It became geographical by absorbing Cambria into Siluria ; and finally, it became sectional by making the great Cambrian series the equivalent of the " Llandeilo formation !" Greater geographical and sectional portents were never brought forth during the history of physical science. The nomenclature does not mask, but greatly exaggerates the incongruity of the scheme ; and it contains within itself the inevitable elements of its own destruction.

The Corndon sections are appealed to in defence of the classification and nomenclature. Unfortunately, the fossiliferous beds of Coradon were called Llandeilo flag ; and hence, if the typical Llandeilo group had a wrong place in the Cambrian series, the Corndon beds could not have a right one. It is mere mockery to compare the Corndon series with that of the Tabular View, and to set it up as the Cambrian type. Its parts are defective in succession, obscure in detail, and have only been put into an hypothetical co-ordination with the lower groups of the true and typical series of Cambria.

The only real argument for the " Lower Silurian " nomenclature is derived from the so-called Lower Silurian fossils. They are unquestionably Cambrian. How could they be otherwise, being taken from a low Upper Cambrian sub-group. But to argue from the fossils only without an appeal to co-ordinate sections, is to desert the first principles on which the ' Silurian System ' had its legitimate claim for our acceptance. As a matter of fact, the old Cambrian types of the Caradoc sandstone were gained by a false step over the May Hill beds ; and as for the Llandeilo fossils they became Silurian only by the greatest sectional mistake the

Author of the "System" ever made during his long, well-fought battles in the geological field. As a matter of fact also, it was known that many thousand feet below the level, where in his own sections he had placed his base line, the same old Cambrian types existed in abundance. Hence, from the very first, the Silurian nomenclature was over-ambitious. The so-called Silurian groups (upper and lower) never made a "System :" and the only way to obtain a true sectional and palæontological "System" out of them, is to contract their base to the May Hill sandstone, and then the "System" (if it must be so called) may remain secure.

All the previous details were recently submitted to the Geological Section of the British Association during its meeting at Liverpool; but they were preceded by an illustrated synopsis of the successive groups which form the great Cambrian series. This synopsis and its sectional illustration are here omitted, because they would convey little more than an expansion of what has been already published in the November Number of the Philosophical Magazine (*supra*, p. 362, &c.).

The following questions were prominently offered for discussion.

1st. Was the Cambrian series based on the evidence of true sections; were its subdivisions natural, its names geographically appropriate? Did the sections exhibited make an approach to a good physical analysis of the great Cambrian series?

2nd. Was the evidence for the existence of a May Hill group (as entirely distinct from the Caradoc sandstone, and forming the physical and palæontological base of the Wenlock shale, and the so-called Upper Silurian rocks) well established?

3rd. Were there indications of great mechanical movements anterior to, or during, the period of the May Hill sandstone? Were they made probable, sometimes by the existence of conglomerates, sometimes by a discordancy of position, sometimes by an obvious interruption to the continuity of the deposits? Or in other words, was not the Silurian series frequently unconformable to the Cambrian?

4th. Was there not an immediate and great palæontological change between the fossils of the May Hill sandstone and those of the groups (whatever they might be) on which it rested?

To all these questions I have, directly or indirectly, given an affirmative reply in this or my previous paper.

But what were the replies given to them by the gentlemen of the Government Survey, and by some other distinguished geologists who were present? Every one who spoke had already been in some form or other committed to what I am now certain is an erroneous, and therefore cannot be a tenable nomenclature; and

each speaker *absolutely evaded* the 2nd, 3rd, and 4th questions, and left the obvious inference drawn from my own affirmative answers untouched.   Whether this was done out of forbearance towards myself I must leave the reader to judge.   As a matter of fact my questions were *evaded,* and both the facts and inferences of my paper remained absolutely untouched.

One of the leading members of the Government Survey affirmed that I had only raised a question of words—a question already settled.   I reply that, between us, it is not a question of words but of facts, and of the classification of certain natural groups. If my interpretation of the sections of Castell Craig Gwyddon, of the great group between Dol Fan and Garn Fawr, and of certain groups south of Llandovery be right, the Government Surveyors have misinterpreted them.   If they have understood them, I have blundered in their interpretation.   There is a plain question of fact between us; and till it is settled the discussion is by no means one of mere words.   Again, the discussion of the May Hill sandstone, and of its place in the lower Palæozoic division, offers us no mere question of words, but a vital question of classification, on which absolutely depends the existence of the so-called " Middle Silurian " group of the Government Map.

More than one speaker suggested the separation of Cambrian from Silurian rocks by a line drawn immediately above the Lingula flags.   I doubt the possibility of finding such a line; and were it possible, it would be of no value to classification. The slates of Festiniog are as true Cambrian slates as those of Nant Francon.   Each of these slate groups forms a great and characteristic Cambrian sub-group, one of them is above, and the other below, the Lingula beds, and neither of them is seen in Siluria.   The only natural break in our older palæozoic series is where I have placed it—immediately below the May Hill sandstone, which is both the physical and palæontological base of the Silurian series.

One thing was admitted, that my *Cambrian series* was physically and geographically true, and that I was the *first person* who had worked out its analysis.   If it be so, who, I ask, has a right to change my nomenclature?   Certainly no one, unless he can somewhere find a more perfect type of development, and a better base for a more perfect nomenclature.

Finally, at the end of a second day's discussion, I obtained by a direct question, this acknowledgement from the President of the Section, that he admitted the May Hill sandstone as a true palæontological and physical group.   On this point, therefore, the two palæontologists of the Government Survey are now with me; while once on this very important point they were my stout opponents.

Historical remarks upon the origin of the Cambrian and Silurian controversy.

The " *Silurian System,*" with its beautiful illustrations and great list of well-determined fossils, is known to all geologists ; and I have already shown that in all its upper subdivisions it is based on most clear and perfect evidence. On the contrary, that in its two lower divisions it fails, not merely from defect of evidence, but also from a positive misinterpretation of the fundamental sections on which the " System " was built.

The evidence on which I have endeavoured to establish and define the *great Cambrian series* is open to no such fatal objection ; but this evidence is still imperfectly known ; partly because it has been given in a synoptical and scattered form, and partly because it has been misunderstood or strangely misrepresented. The whole series was made out in 1831 and 1832; and if we strike out the reference to the Llandeilo flag and the fossil Lingula, the descriptive explanation of the. Tabular View (*supra,* p. 362) might be applied, word for word, to the laborious and (I dare to say) successful work done by myself during nine months of those two summers.

In 1831 I made out the relations of the Bangor group and the Harlech grits; and in the absence of fossils, the grits afterwards became my base line of comparison between the systems of Carnarvonshire and Merionethshire. I also made out the symmetrical undulations of the Snowdonian chain, and the sectional position of the Snowdonian fossiliferous trough, which is several thousand feet above the Harlech grits. These facts, with the sectional evidence from which they were derived, were exhibited and described to the British Association in 1832. During the summer of 1831 I also laid down, from actual survey, the two great parallel bands of porphyry east of the Menai, all the protruding syenitic bosses in south-western Carnarvonshire, and the zone of metamorphic slate on its coast*.

I had no difficulty in interpreting the contemporaneous trappean beds, and the trappean shales or *schaalstein* of the Carnarvon chain, having learnt that lesson long before in Cumberland;

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* My geological map of Carnarvonshire (with the colours afterwards transferred to the Ordnance Map by Mr. Salter) has many times been on the walls of the Geological Society. Mr. Darwin found fossils at Cwm Idwal in 1831. They occur in the prolongation of the Snowdonian trough, which had long been known to contain fossils, and were shortly afterwards collected by myself. I never attempted to define the *exact* place of the Snowdon fossils in the great Cambrian series ; but in 1832 I thought it *probable* that they were below the Bala limestone, because they were at a much less sectional distance from the Harlech grits of Nant Francon than was the Bala limestone from the same grits near the Merioneth anticlinal.

and taking up in 1832 the task where I had left it in 1831, I crossed other nearly symmetrical undulations till I established the great Merioneth anticlinal.   It is the key to the structure of North Wales, whether we take sections north and south from the apse of the anticlinal to the mountains of Festiniog, or make parallel east and west sections from different parts of the anticlinal across the Berwyn chain, and thence to the carboniferous or triassic groups on the English frontier; for we have a base of Llanberris slates and Harlech grits whereon to construct the sections*. From this base, then, I constructed a section over Great Arenig to the Bala limestone, and from that limestone over the Berwyn chain, near the line of the Llanrhaidr road; and a second section further south so as to pass down the Llanwdden valley; and they gave similar results.   From the base line, or Merioneth anticlinal, to the Bala limestone is an ascending section of enormous thickness.   *Above the limestone* the section on the first line is still ascending for some thousand feet, when the eastern dip is reversed so as to throw the beds into a synclinal form, on the eastern side of which, and just beyond the watershed of the chain, the Bala limestone is again brought out, beyond which we have a descending section through inferior slate and porphyries. The second (or Llawdden) line repeats the synclinal, and brings out the Bala limestone far west of the southern Berwyn watershed.   There are other complicated phæ-nomena in this section which I cannot dwell on here†.

In like manner, during 1832, I examined the northern part of the Berwyn chain. Older Cambrian rocks are there seen on the highest crest; and then after a great flexure and perhaps *fault*, are overlapped by a great series of the upper Bala beds, which give an ascending section with a northern dip as far as Corwen. I afterwards determined the place of a great group composed of sandstones, slates, and more rarely of conglomerates, which form the base of the Denbigh flag, and, along with that flag, range in a position discordant to the Bala rocks. This group I believed to be immediately superior to the Cambrian slates of Corwen, though the sections were broken and discontinuous.

All the facts and inferences above stated were discussed, and

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* By *Harlech grits* I mean a remarkable stage of coarse siliceous sand-stone, sometimes almost a conglomerate, which overlies the great Llanberris and Nant Francon slate quarries.   Its finest exhibition is in the Rhinog Fawr chain between Festiniog and Dolgelly. In the Government Survey it is called Barmouth sandstone; and if I mistake not, includes a larger group of strata than I include under the Harlech grits. As the typical grits under Harlech Castle are a *detached* mass of the rock, I am not sure that my name *Harlech* was well-chosen.

† Such *e. g.* as the repetition of the Bala limestone by a fault, and the entire inversion of the beds at the eastern end of the section.

amply illustrated by sections, in a paper read before the Geological Society in 1838 (Proceedings, vol. ii. p. 679, 680). The paper was not published, for the simple reason that I could not bring the upper portion of the sections into any intelligible coordination with the lower groups of the " Silurian System :" and I quote the sections in this place only to prove that the Cambrian series (of course adopting by anticipation the names afterwards agreed upon) was, as to its great physical framework, as well known by me in 1832 as it is now*; the separation between the upper and lower divisions of the series being then drawn at the base of the Bala limestone.

I had not (in 1832) connected the upper Bala beds of Corwen with those of Glyn Ceiriog by a complete traverse, but there could be little or no doubt of their continuity and identity. I did, however, make some elaborate sections to the east of the Berwyns, connecting the fossiliferous group of Glyn Ceiriog with that of Meifod. They are separated by a rude irregular saddle of old Cambrian rocks; each of them is overlaid by what was even then called Wenlock shale, and their fossils are identical. There can be no doubt that, considering the facts by themselves, any observer would have connected the Meifod beds, the Glyn Ceiriog beds and the upper Bala beds into one geological group; and if this be done, the great Cambrian series becomes complete without blemish or hindrance.

Finally, in 1832. I made traverses from the south flank of Cader Idris as far as the banks of the Dyfi, but with little profit; for I failed to find the object I was seeking, viz. the Bala limestone, which was the hoped-for base for another summer's work. One positive result was, I believe, gained. The first dark and impure slate group overlying the Cader Idris system, was identified with a corresponding group (Upper Cambrian *a* (1), *supra*, p. 362) of the Arenig section; and a similar group overlies the old slates and porphyries of Pembrokeshire, as I

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* The dark earthy slates of Carnarvon were at first left by me in a *doubtful* place; but I was the first observer who put them in their right place and made them the equivalents of the Tremadoc slate (*supra*, p. 362). When the Lingula was found by Mr. Davis near Tremadoc, I knew its sectional place in the series, and I had no difficulty whatsoever in afterwards following it from Tremadoc to Cader Idris. Some time afterwards I wrote to a friend in the Government Survey (either Mr. Salter or Mr. Jukes), and stated that if my conception of the structure of the Carnarvon chain was right, the Lingula beds ought to be found immediately over the Harlech grits, which cap the great slate quarry of Nant Francon. The reply was, that they had found the Lingula in the beds referred to. I mention these facts, not in the way of boasting, but to save myself from the imputation of having dressed up the Cambrian series at a period long after 1832. My Cambrian sections were right, or very nearly right, from the beginning; and there was no great mistake in my sections of 1838.

found in 1836*. In a country like South Wales we may rejoice if we can but take one safe step. I then made two complete traverses across the Upper Cambrian groups of South Wales, between the Silurian terrace and the sea, still looking and inquiring for a trace of the Bala limestone, but in vain. But I did gain what I thought good evidence to prove, that the equivalent of the Bala limestone, whatever form it now took, was not far from the base of the great undulating series of South Wales ; and from this opinion I have never swerved.

To examine the Llandeilo sections of the Towy was not then my object, but I crossed two lines, pointed out to me by Murchison, which seemed to connect the Llandeilo Flag with the base of the Wenlock terrace. This done, I made a hasty traverse through a portion of the South Welsh coalfield, and so ended the labours of one summer,—the hardest I think of my geological life.

In 1833 I was, during the early summer months, crippled by illness and incapable of taking the field ; but I saw the beautiful Silurian sections which were exhibited before the British Association by the author of the ' Silurian System ; ' and I also gave a short summary of my results in Cambria as above described ; and I remarked that our work seemed to be interlaced on the east side of the Berwyns, and that we could not connect our groups together without some joint labour in the field.

In 1834 we did visit, together, the best Silurian sections from Caermarthenshire to Denbighshire. I believed the sections on what appeared the evidence of demonstration ; they were the most clear and striking I had ever beheld ; but I never saw their base, or considered their relations as admitting of any doubt. I accepted them as they were offered in the supposed Silurian order of superposition (fig. 5. p. 313). But when we reached the sections on the eastern side of the Berwyns, a country I had previously examined, I became startled at the conclusions which followed. The Meifod and Craig-y-Glyn beds were pronounced to be Caradoc and Llandeilo†. Now if the Meifod beds were Lower Silurian, then the Glyn Ceiriog beds must also be Lower Silurian. But the Bala limestone and calcareous slate were not Lower Silurian, because they underlaid the great Upper Cambrian series of South Wales, *over* which, in the Silurian sections, were placed the Lower Silurian rocks. On this point there was, in fact, no difference of opinion ; for, as a conclusion from the Berwyn sections alone, my friend excluded the Bala limestone

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* The visit in 1836 was made for the purpose of comparing the Culm trough of Devon with that of South Wales. When my friend and fellow-labourer had left me, I devoted a few days to the older Cambrian rocks of Pembrokeshire.

† See sections of the Silurian System, pl. 32. fig. 9.

from his Lower Silurian groups. The Bala limestone must therefore (if the Lower Silurian sections were right) be put in a lower system than that of Glyn Ceiriog. This threw my Upper Cambrian sections into great confusion; for it is only by a distortion of the Geological Map that the Ceiriog limestone can be thrown out of co-ordination with that of Bala. An error, and a great error there *was;* and it must have arisen from some *great mistake,* either in my Upper Cambrian, or my friend's Lower Silurian, sections. I *provisionally* accepted the former alternative, greatly to my own cost; for it led me inevitably into errors in the estimate of the older rocks of the north of England, by putting before me what I afterwards knew to be an erroneous Silurian type*. So far from the "Lower Silurian" groups being a key to unlock the mysteries of our older palæozoic rocks, they became a bolt to close the door of progress.

On my next return to North Wales in 1842, I found that all my Upper Cambrian sections had been true to nature; but a mistake there was as I could prove to demonstration, and *that mistake* must now, therefore, be attributed to the right quarter, —to a great mistake in the original Silurian sections.

What took place afterwards needs no long comment. My *Upper Cambrian* groups (which I have *since* extended downwards to the flank of the Arenig porphyries as in the Tabular View) became *interlaced,* as I had suspected in 1833, with the *Lower Silurian*; and all the entanglement which afterwards arose sprang from no mistake whatsoever in my *Upper Cambrian sections,* but from a great mistake in the *Lower Silurian.* I therefore proposed a compromise, viz. that all the groups from the Bala limestone to the base of the Wenlock shale should be called Cambro-Silurian. I had placed those groups correctly in the great Cambrian series, but had said little of the fossils; except that, so far as I

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* Should any one think it strange and almost incredible that I should thus have distorted my own sections to bring them into accordance with the Lower Silurian scheme (*supra,* fig. 5. p. 313), let him bear in mind (1) that the Glyn Ceiriog calcareous slates are immediately overlaid by the Wenlock group, and that the Bala limestone and its calcareous slates are overlaid by several thousand feet of strata bearing neither physical nor fossil analogy to the Wenlock group. (2). That on this sectional ground the Upper Bala group was also separated from those of Glyn Ceiriog and Meifod by the author of the 'Silurian System.' (3). That there *is a true Silurian group* to the west of Corwen and on the line of the Holyhead road, which is based on the May Hill sandstone. What I supposed in 1834 (when I for the last time parted from Sir R. I. Murchison in North Wales) was this, viz. that the Glyn Ceiriog limestone *might be* prolonged to the base and made a part of the group I now call May Hill sandstone. It was an *hypothesis* to which I was absolutely driven, provided the Lower Silurian sections were, as I then believed, perfectly true to nature. I had no time that summer to test the *hypothesis.*

knew them, they were the same as the "Lower Silurian" fossils; while my friend had described and figured numerous fossils from two sub-groups, but had greatly mistaken their place in the general section of North Wales. I also suggested as a compromise the adoption of the name *Protozoic* (first thrown out by Sir R. I. Murchison himself) for the whole Cambrian series,— it being expressly understood that wherever the demarcation could be drawn, all the groups above it (to the "Upper Silurian" base) should be regarded as Cambro-Silurian,—all below it, as Cambrian. This compromise was rejected; for my friend, without a single word of communication to myself, brushed out his base line, and coloured all the older palæozoic rocks of North and South Wales as "Lower Silurian."

Other schemes of Nomenclature, &c.

Before I bring these discussions to an end, I wish the reader to bear in mind, that the observations of 1842 and 1843 did not invalidate, but confirmed the truth of the whole Cambrian series, as established by myself in 1831 and 1832. On the contrary, that the same observations made it almost certain that the relations of the "Lower Silurian" groups of South Wales to my Upper Cambrian groups were misunderstood and misrepresented in the Silurian sections. Hence it was that I thought I had proposed a very generous compromise, viz. that the name *Upper Cambrian* should disappear, and in its place we should use *Cambro-Silurian*—the name *Cambrian* being confined to the groups *below the Bala limestone*; and that in those places where the subdivisions could not be established on evidence, the whole fossiliferous series below the Wenlock shale should be called *Protozoic*—that term including both the fossiliferous Cambrian and Lower Silurian groups. (Proceedings of Geol. Soc., vol. iv. p. 223.)

But in the same volume of the Proceedings (p. 251) is another abstract with my name affixed to it. In point of fact, it is an abridgement of two long papers recording the observations of Mr. Salter and myself, made both in 1842 and 1843. It was composed by the President of the Society, and I was *not permitted,* after repeated applications, even to see the proof sheets. It was got up, I doubt not, with much care; but what becomes in it of my proposed compromise? In the accompanying map, I am stated to make *Protozoic* the equivalent of *Lower Silurian*! I never committed an act so suicidal: indeed I had most carefully, as I thought, fenced myself against any such inference, as appears by a reference to the *Proceedings* (vol. iv. p. 221). One of the definitions in my reduced map was *changed without my knowledge*: and strange as it may seem (never

suspecting that my proposed nomenclature had been most *unwarrantably tampered* with), I was for some years perfectly unconscious of the fact, never having scrutinized the reduced map, or read its definitions of the several geological subdivisions.

Having before referred to this strange passage in the history of a controversy, I should not again have taken it up, except to introduce one or two remarks on the classification of the Government Survey.—(1) Under the name *Cambrian* they include only the Lower Cambrian group of the Tabular View (*supra*, p. 362). (2) Under the name *Lower Silurian* they include all the Middle Cambrian and nearly all the Upper Cambrian groups of the Tabular View. (3) Under the name *Middle Silurian* they include the *Caradoc sandstone*, as well as all the groups I have since called *May Hill sandstone*. (4) Lastly, they naturally adopt the Upper Silurian groups, about which there never has arisen any doubt.

In this scheme I think that the separation of the first and second subdivisions into Cambrian and Silurian is unnatural; and that, so far, the nomenclature in the new work 'Siluria' (though wrong in principle) is more consistent than their own.

In the next place, I think their second division has only been established by a desertion of the principles which the Director of the Survey has repeatedly advocated in the Geological Society, with, perhaps, more energy than any other member of it, viz. that in every new case of classification we must ground our system on the succession of physical groups as well as on the evidence of fossils. This true principle is also energetically asserted by Sir R. I. Murchison in his Anniversary Address for 1842 (Proceedings, vol. iii. p. 649). "So long (he teaches us) as British geologists establish a classification founded on the sequence of the strata, and the imbedded contents, ... so long will their insular names be honoured with a preference by foreign geologists." But was this plan pursued in the downward extension of the Lower Silurian groups, either by himself or by the gentlemen of the Government Survey? I am certain it was not followed out by himself; and I think it was not followed out in the Government Survey. They soon found that the "Silurian System" was on an erroneous base, and that the Llandeilo flag was in a false relation to the "Upper Cambrian" groups of South Wales. But having reduced it to its right relation, they then developed it upwards through five or six thousand feet of strata, and downwards through more than twice as many thousands, and so elaborated their second division with a nomenclature which is geographically incongruous, and had no primary reference to the actual succession of physical groups; and they did this while a truer and more philosophical nomenclature was before them.

I believe the Director of the Government Survey thought that I had abandoned my old nomenclature (and well might he think so after what I have stated respecting the *tampering* with the names of my *reduced map*), and that the Silurian nomenclature was to be taken as a fact established ; and hence, that the primary object of the Surveyors was to follow out, and give meaning to, the Lower Silurian groups.   Be this as it may, they did not keep their nomenclature in abeyance till they had worked out all the physical and palæontological subdivisions of the Cambrian series ; and hence their successive groups have a very great geographical inconsistency, and were not all palæontologically right.

Thirdly. The Middle Silurian group of the Survey has lost its meaning since the establishment of the May Hill group.   On this enough has been said before.   The May Hill group is, in the nomenclature of this paper, not an Upper Cambrian, but a true Silurian group.   It cannot therefore be called Upper Caradoc without the great inconvenience of leading us to a false association, and also to a locality where the section does not give us a good May Hill type.

Though the great Cambrian section (which is in the first place derived from the beds on the west side of the Carnarvon chain, and ascends to the Harlech grits, and is afterwards taken up at the representative of the same grits near the Merioneth anticlinal, and is thence carried across the Berwyns) is not geographically, yet it is geologically, continuous.   The section on the east side of the great Arenig gives us a beautiful and uninterrupted Bala series, which we do not see in the Upper Cambrian groups of South Wales : yet the Bala series is defective at the top, where it is overlaid, I believe unconformably, by the May Hill sandstone.   In this respect the Upper Cambrian sections of South Wales are not only, I think, thicker, but much more perfect in the ascending order than those of North Wales ; and this fact, if I rightly interpret it, is explained by those movements of elevation which produced among other phænomena the conglomerates and coarse sandstone on the eastern skirts of the Radnor mountains; and were succeeded by the unconformable deposits at the base of the Upper or true Silurian rocks.

When I crossed South Wales in 1846, I still thought it possible (giving it a *very broad margin*) to construct a *Cambro-Silurian* group which should include the Llandeilo flag, and yet leave several older slate groups to the west of it, before we reached the shores of Cardigan Bay, or the flank of Cader Idris. I now abandon that hypothesis ; and since the establishment of the *May Hill group*, the name *Cambro-Silurian* is without any value or geological meaning.

To call all the contorted old rocks to the east of Cardigan Bay

by the name Llandeilo flag, is to blink, and not to solve, the difficulties of a most intricate and perplexing country; but to determine the western emergence of the Llandeilo flag of the Towy (whatever be the mineral type it puts on) would be a real step in physical geology. Nor is the country altogether without fossils; for example, in the district north of the Devil's Bridge, and in the upper valley of the Cothi, where to the well-known *Diplograpsus pristis* M'Coy added a second old Cambrian graptolite. Nor should I yet despair of finding May Hill or Wenlock beds among the great folds of South Wales; but I must not lengthen out a paper already far too long, by enlarging on speculations and hypotheses.

Once for all, then, I confidently affirm, that the great Cambrian series is a natural and true series, and that its subdivisions have been rightly named. On the other hand, I contend that the classification and nomenclature of the same series in the scheme of the Government Survey is erroneous and untenable. 1st. Because it overlooked the true physical and palæontological base of the Silurian series; 2nd. Because its nomenclature is but an expansion of Silurian names derived from sections which were not true to nature. 3rd. Because no section, within the limits of Siluria, gives us a good older Palæozoic type. 4th. Because the nomenclature of the Cambrian series (as given in the Survey) was not deduced from the natural groups; but, on the contrary, the natural groups were so packed as to fall in with a previous (Silurian) nomenclature, which was based on erroneous sections. 5th. Because it introduces two different and incongruous schemes of nomenclature into one system, and consequently involves the nomenclature in most needless geographical contradictions and incongruities. There ought never to have been any controversy on the questions I have been discussing; and since the establishment of the May Hill sandstone, there is now at least no shadow of reason for its continuance.

Conclusion.

Before I close this paper permit me to notice, as shortly as I can, one or two points which do not affect the conclusions I have vindicated, but do belong to the history of the controversy, and to my conduct in it.

1. I was not the aggressor. For years I did not care a straw for any little points of difference, or any mistakes I might have remarked upon. For I felt certain that I had the older palæozoic series of England well in hand, and I did not believe it possible that any one could dispossess me of it; or if he could show good claim to any part of it, so far I was ready, either to give way, or to go shares with him. It was only when my

whole work was sponged out of the map of Wales, and treated as if it had been but one great blunder, that I defended myself, and in so doing, vindicated the truth : for I made no great blunder, except when I *doubted* about a small part of my own Cambrian series, which turned out to be right, and believed in the sectional truth of a part of another series, which turned out to be wrong.

2. When I first complained a little of my friend's want of courtesy in having invaded the two Principalities without any previous declaration of war—in having brushed out his own boundary line, and incorporated all the older parts of North and South Cambria in his newer state of Siluria, I was met then (and I am still met) by an appeal to a passage in the anniversary speech to the Geological Society for 1843 (Proceedings, vol. iv. p. 73). I will quote the passage, word for word. "We were both aware, and the point was fully commented upon in my own work (Sil. Syst. p. 308), that the Bala limestone fossils agreed with the Lower Silurian ; but depending upon Professor Sedgwick's conviction, that there were other and inferior masses also fossiliferous, we both clung to the hope that such strata, when thoroughly explored, would offer a sufficiency of new forms to characterize an inferior system." I think it best to quote my own reply to this passage, as it appears in a fifth letter on the Lake District, dated June, 1853*. It is as follows :—" In omission and commission, it (*i. e.* the passage above quoted) is a virtual misstatement of the facts. The author does not inform the reader that he had himself consented in 1834 to put the Bala limestone in my Upper Cambrian groups,—Because it had a sufficiency of new forms to mark a new system ? By no means; but because it was the base of a great physical group which he himself had excluded from his own system in South Wales, and *over which* he had (erroneously, as was *afterwards* made out by other observers) placed his Llandeilo group. Nor does he tell the reader that I had, from the first, strenuously opposed the word *system* when applied to the (collective) Silurian groups ; because they had no defined base, either physical or palæontological. The sentence quoted proves to demonstration that my objection (and I may add the repeated objections of Professor Phillips) to the word system (as applied to the Silurian groups) had been right, that the Silurian nomenclature was in abeyance, and that it must be considerably modified in order to bring it into any conformity with a true geographical nomenclature, and with the palæontological evidence of more complete sections."

Another paragraph of my letter is as follows:—" When the author states 'that we both clung to the hope that the Cambrian

* Guide to the Lakes, by J. Hudson. Kendal, 1853.

groups would offer a sufficiency of new forms to characterize an inferior system,' I can only reply, *that the hope to which he clung* was not derived from anything I had ever said or written; and that I had not, in 1842 and 1843, the shadow of a hope that any new system of animal life, any group of new forms 'marking an inferior system,' would be found among the Lower Cambrian groups. I had constantly expressed, and repeatedly published, *a directly contrary opinion.*"

I then quote the fact (published in Jan. or Feb. 1837) that my small collection of the *most common and characteristic* species of the Bala limestone, was found by Mr. Sowerby not to have as much as one which was not in his MS. Silurian lists. Again, in 1838, I state (in the Proceedings of the Geol. Soc.), when writing of the Upper Cambrian group, "that many of its fossils are identical with those of the Lower Silurian rocks," "that *no distinctive zoological characters* had been well ascertained." Other like quotations are given in the letter: *e. g.* where I state that a small collection I had procured from the Snowdonian trough, when examined in 1841, gave only Bala species, &c.

When the reader bears in mind that the passages referred to were all published *before I had revisited* North Wales, while I believed in the integrity of the Lower Silurian sections, and therefore also inevitably believed that the Bala limestone was several thousand feet below the Llandeilo flag,—bearing all this in mind, he must, I think, conclude that the author of the 'Silurian System' had but small reason for attributing to me *that hope to which he himself had clung.* Nay, rather the reader must conclude (as I have done in the letter quoted) "that the human mind will sometimes reach *a foregone conclusion* without any remembrance of previous facts, or any exercise of the inductive faculty."

Had I acted in a controversial spirit, I should have immediately answered (as I am now doing) the passage contained in the President's speech. My paper, read four months afterwards, was, however, the proper and amicable refutation of the passage, so far as it did not truly represent my real and well-grounded hopes or anticipations.

3. Mr. D. Sharpe, in a paper on the Bala limestone (Proceedings of the Geol. Soc. vol. iv. p. 10), states "that Professor Sedgwick placed the Bala and Coniston limestones in the Upper Cambrian system, which he states to lie below the Silurian system of Mr. Murchison, and above the Lower Cambrian system; a view adopted by Mr. Murchison in his work on the Silurian system, upon the authority of Professor Sedgwick." The assertion, implied in the concluding part of the extract, that Mr.

Murchison borrowed from me, or took on my authority, his views respecting the relation of his Lower Silurian groups to the Upper Cambrian system, is so directly contrary to fact, that I can only oppose it by a direct denial.   But I by no means accuse Mr. Sharpe of intentionally misrepresenting me.   I have often spoken of the great Upper Cambrian groups of South Wales as inferior to the Silurian system ; but on what authority?   On the sole authority of the Lower Silurian sections, and of the author's many times repeated explanations of them before they were published. So great was my confidence in his work, that I received it as a perfectly established truth that his order of superposition was unassailable.   Not one atom of blame, touching this question, can rest upon me.   I never gave advice about it, and I never was consulted upon it.   I accepted his order of superposition and clung to it, and never doubted of its truth, till in 1842 I had followed it into one of its remote consequences; and then found that it could not be true, because it involved in its consequences a palpable contradiction.

I asserted again and again that the Bala limestone was near the base of the so-called Upper Cambrian group.   Sir R. I. Murchison asserted, and illustrated by sections, the unvarying fact that his Llandeilo flag was superior to the same Upper Cambrian group.   There was no difference between us till his Llandeilo sections were proved to be wrong.   But I need not dwell on a point that cannot, I trust, be disputed, as it would at length involve a direct question of truth and falsehood.

The author of the ' Silurian System ' cannot have overlooked the passage last quoted, and he ought not to have allowed it to pass unnoticed, in the comment on Mr. Sharpe's paper, which appeared in his next anniversary address.   It is not always easy to comprehend an author's meaning ; but there are some passages in the recent work ' Siluria,' which an ordinary reader would, I think, misinterpret, and so construe as to infer that in describing the Cambrian groups I had misled the author of the ' Silurian System.'   I did not mislead him, unless it were by implicitly adopting his views and believing in his sections.   The " System " is before the world.   Its groups are all his own ; and if he mistook the relations of his lowest group, and thereby put a drag (as assuredly he did for several years) upon the progress of geology, he has honour and strength enough to bear this blame ; and it would ill-become him to throw one particle of it on the shoulders of his old friend and former fellow-labourer*.

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* There are some sentences which I greatly object to in the recent work, " Siluria" (Chap. i. pp. 7–9).   They *do not state the whole truth* ; and without some comment, they naturally lead to a conclusion which is not historically true.   They will, I trust, be set right in the next edition.

LXI. *On some new Laws of Reflexion of Polarized Light. By the* Rev. Samuel Haughton[*].

IT is well known to experimental opticians, that the laws of polarization of plane-polarized light reflected from the surfaces of transparent bodies, given by Fresnel and Brewster, are only approximate laws, except in the case of some glasses of a certain refractive index and reflecting power, in which the laws of Brewster and Fresnel become as exact as can be expected in such experiments. But although the fact that these laws are only approximate was well known, it was not until the experiments of M. Jamin were published that we had a numerical statement of the extent of deviation from these generally received laws. These experiments are published in the *Annales de Chimie et de Physique,* vol. xxix. p. 263, and must be regarded as a model of accurate and unprejudiced observations of facts. The facts ascertained by M. Jamin in his examination are summed up by him as follows:—

1st. That almost all solid substances polarize light imperfectly by reflexion.

2nd. That they transform an incident plane-polarized beam into a beam elliptically polarized.

3rd. That if the incident plane-polarized beam be resolved into its two components, in and perpendicular to the plane of incidence, the difference of phase in the reflected components, at the perpendicular, principal, and grazing incidences, is 180°, 270°, 360°, respectively.

4th. That the laws of reflexion depend on two constants, one of which is the index of refraction, and the other the coefficient of ellipticity arising from the reflecting power of the body.

5th. That all substances, whose index of refraction is greater than 1·46, accelerate the phase of the component in the plane of incidence.

6th. That all substances, whose index of refraction is less than 1·46, retard the phase of the component in the plane of incidence.

7th. That substances whose index of refraction is sensibly equal to 1·46, polarize the reflected light rectilinearly, and obey Fresnel and Brewster's laws.

8th. That M. Cauchy's formulæ for the diamond apply to all bodies, while Fresnel's apply only to the particular case of reflected light which is plane-polarized.

9th. A table, p. 303, contains the constants of elliptic polarization of forty-three substances.

In a paper published by me in the Philosophical Magazine, Fourth Series, vol. vi. p. 81, I have shown that the 8th statement

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is not peculiar to M. Cauchy's formulæ, but that Mr. Green's formulæ (subject to a modification which I have suggested) explain the facts at least as well as M. Cauchy's.

Since writing the paper just mentioned, I have had an opportunity of trying many experiments with an apparatus similar to that used by M. Jamin, and fully described by him in pp. 271–276 of the memoir already quoted. To avoid repetition, I refer the reader to M. Jamin's description, and shall proceed at once to a description of the new facts which I have been fortunate enough to ascertain.

It occurred to me, on reading M. Jamin's memoir and repeating some of his experiments, that it would be instructive to ascertain the position of the major axis of the reflected elliptically-polarized light, and to tabulate the ratio of the axes of the ellipse for different conditions of incidence and azimuths of incident plane-polarized light. I accordingly investigated formulæ for the position and ratio of axes in terms of the quantities measured by direct experiment. The details of this investigation are given in the next section, and the details of the experiments in the following.

The method followed by me is purely experimental, and independent of any theory as to the nature of light; by following this method rigorously, and faithfully adhering to the results of observation, I have succeeded in establishing the following facts or laws, which, so far as I am aware, are new, and are certainly interesting.

Law I. That if plane-polarized light be incident on a transparent reflecting body, and the angle of incidence be gradually increased from 0° to 90°, the ratio of the axes of the reflected elliptically-polarized light diminishes from infinity at 0° to a *minimum* attained at the principal incidence, and increases again to infinity at 90° incidence.

Law II. That this *minimum* ratio of axes diminishes as the azimuth of the incident plane-polarized light (or angle made by its plane of polarization with the plane of incidence) approaches a certain value, which can be ascertained, and which I shall call the *Circular limit*.

Law III. That when the azimuth of the incident plane-polarized light reaches this value or limit, the reflected light is *circularly polarized,* and the ratio of axes is unity.

Law IV. That for azimuths of incident plane-polarized light greater than this limit, the *minimum* ratio of axes increases, becoming greater as the incident azimuth departs from the circular limit.

Law V. That when the azimuth of the incident light is less than the *circular limit*, the axis major of the elliptically-polarized

reflected light moves as in the annexed figure. Let POA be the azimuth of incident light, and POA equal to GOA; PO is the position of the axis major corresponding to 0° incidence; OA the position of the axis major in the plane of incidence corresponding to the principal incidence; and

Plane of incidence.

OG the position of the axis corresponding to 90° incidence.
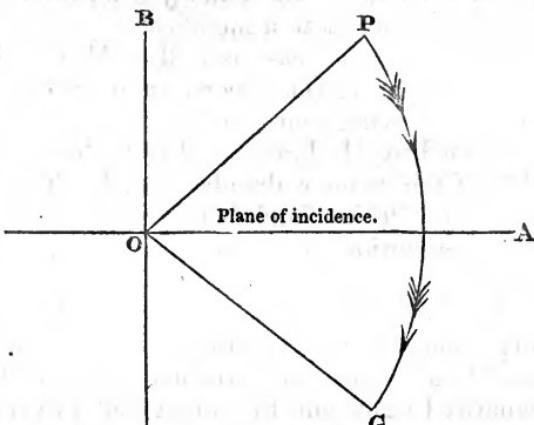
Law VI. That when the azimuth of the incident light is greater than the *circular limit*, the axis major of the reflected light moves as in the annexed figure. Let POA be the azimuth of the incident light, and GOB equal to POB.

Plane of   O   incidence.

At the incidence 0° OP is the position of axis major; as the incidence increases from 0° to the principal incidence, the axis major moves from OP to O*x* and turns back, attaining the position OB at the principal incidence; and as the incident angle increases from the principal incidence to 90°, the axis major moves from OB to O*y* and back again to OG.

Previous to entering upon a description of the experiments by which the foregoing six laws are proved, it will be useful to show by an example the manner in which the received laws of Fresnel and Arago deviate from the observed facts.

According to Fresnel, if a unit of light be polarized in the plane of incidence and reflected from an uncrystalline transparent body, the intensity of the reflected beam will be measured by the square of the fraction

$$I = \frac{\sin (i - r)}{\sin (i + r)}, \quad \cdots \quad (1)$$

i and *r* being the angles of incidence and refraction. If the unit of light be polarized in a plane perpendicular to the plane of in-

cidence, the intensity of the reflected light will be measured by the square of the fraction
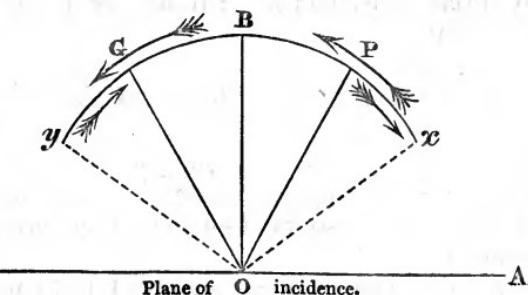
$$J = \frac{\tan(i-r)}{\tan(i+r)}. \quad \cdots \quad \cdots \quad (2)$$

From these expressions it follows, that if a beam of light be polarized in a plane making an azimuth A with the plane of incidence, and if A′ be the azimuth of the polarization of the reflected beam,

$$\tan A' = \tan A \frac{\cos(i+r)}{\cos(i-r)}. \quad \cdots \quad (3)$$

If we now write $Q = \frac{\tan A}{\tan A'}$, it is evident from (3) that the value of Q obtained by measurement of A and A′ should be constant if Fresnel's laws be true, and that it will not be constant if the law be inexact.

It is easy from (3) to deduce the following formulæ, which will serve to calculate the refractive index from measurements of A and A′:

$$\left. \begin{array}{l} \tan r = \dfrac{Q-1}{Q+1} \cot i \\[2mm] \mu = \dfrac{\sin i}{\sin r} \end{array} \right\} \quad \cdots \quad \cdots \quad (4)$$

The following experiments were made with a rhomb of glass made in Munich, the refractive index of which, corresponding to a red very near the extreme red, was found to be 1·6229. The experiments were made with white lamp-light from a Moderateur lamp burning colza oil, and provided with a parabolic silvered reflector; the polarizer consisted of a Nicol's prism, and the analyser of a similar prism, made for me by M. Dubosc, and without sensible deviation.

Table I.
Incidence = 39° 22′.

A.	A′.	Q.	μ.
1̊5	7̊ 15	2·106	1·593
30	14 24	2·249	1·495
45	22 45	2·385	1·421
60	35 39	2·415	1·407
75	56 0	2·517	1·363

The values of Q in this table are found from those of A and A′ in the first two columns, and the values of μ from equation (4); both should be constant according to Fresnel's law, but it is plain from the table that such is not the case.

Table II.

Incidence = 69° 37'.

A.	A'.	Q.	μ.
15	354 45	−2·916	1·550
30	347 0	−2·501	1·431
45	337 0	−2·356	1·385
60	323 15	−2·319	1·372
75	301 0	−2·242	1·347

In this table, with an angle of incidence greater than the polarizing angle, we observe the same facts as before, viz. that Q and μ are not constant but variable; and that not in an irregular manner, but according to some law. The value of μ is nearest to the refractive index in both tables when the azimuth of incident polarization is least, and diminishes as the azimuth increases, being in all cases less than that of the refractive index found in the usual way by refraction.

It is impossible to believe that the law contained in equation (3) can be accurate, when we find it leads to a *regular deviation* from the results of observation. It is not that the values of μ do not coincide with the refractive index, some being greater and some less, in which case we might suspect some errors of observation; but the deviation is regular and real, and can only arise from the inaccuracy of equation (3).

The azimuth A' is that of the major axis of the reflected elliptic polarization, and, as will be shown hereafter, this polarization sometimes approaches to the circular.

1. *Investigation of elliptically-polarized Light.*

The instrument used by M. Jamin for the examination of the elliptically-polarized light reflected from transparent bodies, differs from that used in the preceding experiments by the addition of a compensator placed in front of the tube containing the Nicol prism analyser. This compensator is fully described in M. Jamin's paper, and serves to convert an elliptically-polarized beam into a plane-polarized beam. Its principal planes are in the plane of incidence and perpendicular to it, and the distance to which the moveable prism is moved from the zero serves to calculate the difference of phase between the components of the elliptic beam in, and perpendicular to, the plane of incidence. The plane-polarized beam which passes through it is stopped by the analyser, which reads the position of the polarization of the light which has become plane-polarized by the action of the compensator.

It is required to express in terms of the readings of the ana-
lyser and compensator, the position of the axis major of the
elliptic polarization and
the ratio of its axes. This
may be accomplished by
the following considera-
tions.

Let the elliptically-polar-
ized beam be conceived as
inscribed in a rectangle
whose sides are parallel
and perpendicular to the
plane of incidence OI. Let
Ox be the diagonal of the rectangle circumscribed, and Oy the
axis of the ellipse ; it is required, from the difference of phase of
the light in the planes OI and OP, and knowing the direction of
the line Ox, to find the direction of Oy and the ratio of axes.

It will assist our investigation and involve no hypothesis, to
imagine the ellipse as the path of a point revolving in the direc-
tion of the arrows, whose coordinates may be expressed as fol-
lows :—

$$\xi = A \sin (kt + e)$$
$$\eta = B \sin (kt + e'),$$

where $e' - e$ is the difference of phase between the directions OI
and OP, and A and B are the lines OI and OP.

Eliminating t from the expressions for ξ and η, we find the
equation of the ellipse,

$$\frac{\xi^2}{A^2} + \frac{\eta^2}{B^2} - 2 \cos (e'-e) \frac{\xi\eta}{AB} = \sin^2(e'-e). \quad . \quad . \quad (5)$$

It is well known that if the equation of an ellipse be

$$Dx^2 + 2Exy + Fy^2 = Q, \quad . \quad . \quad . \quad . \quad (6)$$

the position of its axis is found from the equation

$$\tan 2\phi = \frac{2E}{D - F}.$$

Substituting for D, E, F their corresponding values in (5), we
obtain

$$\tan 2\phi = \tan 2\alpha \cos (e'-e), \quad . \quad . \quad . \quad . \quad (7)$$

ϕ being the angle IOy, and α the angle IOx.

Returning again to equation (6), it is not difficult to prove
that if a and b denote the axes major and minor of the ellipse,

$$\frac{b^2}{a^2} = \frac{(D + F) + (D - F) \sec 2\phi}{(D + F) - (D - F) \sec 2\phi},$$
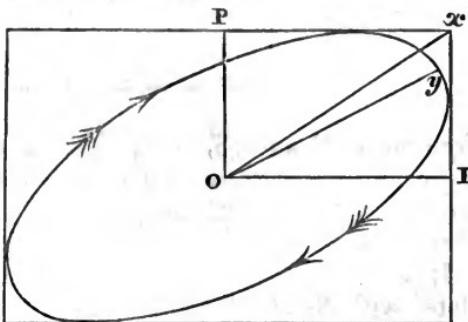
from which and equation (5) we infer

$$\left.\begin{array}{l} \dfrac{a}{b} = \sqrt{-\cot(\phi+\alpha)\cot(\phi-\alpha)} \\[2mm] \dfrac{b}{a} = \sqrt{-\tan(\phi+\alpha)\tan(\phi-\alpha)} \end{array}\right\} \quad . \quad . \quad (8)$$

Equations (7) and (8) serve to calculate the position of the axis major and the ratio of axes from knowing α and $e'-e$; α is the azimuth measured on the analyser, and $e'-e$ is found by means of the compensator as follows :—

It is known from a comparison of the wave theory of thin plates with Newton's experimental laws, that if a beam of light be reflected perpendicularly from a transparent surface, there is a difference of phase of 180° between the incident and reflected light ; a fact which has been admirably illustrated by Dr. Young, by means of the analogy of the impact of elastic balls. The difference of phase, therefore, between the reflected components in, and perpendicular to, the plane of incidence in passing from the incidence 0° to 90°, increases from 180° to 360°, and not from 0° to 180°. By means of the compensator we have the following proportion :—

$e'-e-180° : 180° :: $ the reading of the compensator minus the zero reading : the number of divisions of the compensator corresponding to half a wave length.

The zero of the compensator used by me is 39·43, and the number of divisions corresponding to different kinds of light for half a wave are as follow :—

White lamplight (colza oil) . . .	13·29
Red lamplight (colza oil) . . .	15·43
Red sunlight (same colour) . . .	15·37

If, therefore, C denote the reading of the compensator, we find from the foregoing proportion the equation for red sunlight,

$$e'-e = 180° + (C-39\cdot43) \times \dfrac{180°}{15\cdot37}. \quad . \quad . \quad (9)$$

From (9) and the analyser (α) we calculate ϕ by means of equation (7), and from ϕ and α we calculate the ratio of a to b by equation (8).

The calculations in the following section are all made from these equations.

2. *Account of the Experiments.*

The following experiments, unless the contrary be expressed, were made with the same glass as that used in Tables I. and II., the method of observation being as follows :—The polarizer was

placed at a fixed azimuth, and the angle of incidence increased, as shown in the first column; the actual readings of the compensator and analyser are given in the second and third columns; the values of $e' - e - 180°$, deduced from the compensator by means of equation (9), or a similar equation for the white lamplight, are given in the fourth column; ϕ and $\frac{a}{b}$, calculated from (7) and (8), are given in the fifth and sixth columns; and a seventh column is added containing the values of the angle whose tangent is $\frac{J}{I}$, found as follows:—If A be the azimuth of the polarizer, cos A and sin A will be the components of the incident plane-polarized light in and perpendicular to the plane of incidence; hence if α be the reading of the analyser, we have, since $I \cos \alpha$ and $J \sin \alpha$ are the corresponding components of the reflected light,

$$\frac{J}{I} = \tan \alpha \cot A. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

This column will be found useful in comparing the observations with theory.

Table III.

Azimuth of polarizer $= 45°$.   White lamplight (colza oil).

Incidence.	Compensator.	Analyser.	$e'-e-180°$.	ϕ.	$\frac{a}{b}$.	$\mathrm{Tan}^{-1}\left(\frac{J}{I}\right)$.
43 37	·39·54	18 30	3 11	$+18\ 28$	47·79	18 30
48 37	40·07	10 55	10 17	$+10\ 45$	29·42	10 55
50 45	40·61	8 10	17 36	$+\ 7\ 48$	23·37	8 10
51 45	41·11	6 45	24 22	$+\ 6\ 10$	20·70	6 45
52 45	42·67	6 10	45 29	$+\ 4\ 21$	13·03	6 10
54 20	44·46	5 1	69 44	$+\ 1\ 45$	12·15	5 1
55 20	46·30	5 36	94 38	$-\ 0\ 27$	10·23	5 36
56 20	48·18	6 15	120 6	$-\ 3\ 10$	10·58	6 15
57 40	50·07	7 35	145 41	$-\ 6\ 19$	13·52	7 35
58 40	51·00	9 39	158 16	$-\ 9\ \ 0$	16·16	9 39
60 35	51·60	11 10	166 24	$-10\ 53$	22·54	11 10
65 40	51·98	18 11	171 33	$-18\ \ 2$	22·84	18 11
75 35	52·50	30 25	178 40	$-30\ 25$	Infinity.	30 25

The fifth and sixth columns of this table prove the truth of Laws I. and V.

The value of the principal incidence deduced from the values of ϕ is $55°\ 8'$, at which angle the quantity of light reflected in the plane of incidence (or the major axis of the ellipse) is nearly 104 times the quantity of light in the perpendicular plane (or minor axis); because the quantity of light in the plane of incidence is proportional to the square of the ratio of a to b.

Table IV.

Azimuth of polarizer $=80°$. Red sunlight.

Incidence.	Compensator.	Analyser.	$e'-e-180°$.	ϕ.	$\dfrac{a}{b}$.	$\text{Tan}^{-1}\!\left(\dfrac{J}{I}\right)$.
34 30	39·72	74 0	3 23	+74 1½	88·2	31 35
43 30	40·13	61 45	8 11	+61 53	16·9	18 10
48 30	40·76	49 15	15 34	+49 24	7·62	11 34
50 30	42·15	38 0	31 49	+36 49	3·62	7 51
51 30	42·80	33 30	39 26	+30 36	3·09	6 40
52 30	·43·85	28 0	51 42	+21 17	2·70	5 22
53 30	45·41	25 45	69 57	+11 39	2·28	4 52
54 30	47·20	26 34	90 54	− 0 36	1·99	5 2
55 30	48·80	28 45	109 37	−13 53	2·02	5 31
56 30	50·30	34 0	127 10	−28 7	2·26	6 47
57 30	51·20	40 0	137 42	−38 18	2·64	8 25
58 30	51·21	42 30	137 49	−41 38	2·82	9 10
60 30	52·14	53 45	148 42	−55 8	3·76	13 31
65 30	52·77	66 30	156 4	−67 47	6·59	22 5
70 45	53·18	75 15	160 52	−75 56	12·30	33 49

Here, as before, the fifth and sixth columns prove the truth of Laws I. and V., and a comparison of the column for $\dfrac{a}{b}$ with the corresponding column of Table III. verifies, as far as it goes, Law II.

The principal incidence derived from the fifth column is $54° 27'$, at which incidence the quantity of light reflected in the plane of incidence exceeds that in the perpendicular plane in the ratio of 3·96 to unity.

In the next table the azimuth of the polarizer approaches nearer to the circular limit.

Table V.

Azimuth of polarizer $=85°$. Red sunlight.

Incidence.	Compensator.	Analyser.	$e'-e-180°$.	ϕ.	$\dfrac{a}{b}$.	$\text{Tan}^{-1}\!\left(\dfrac{J}{I}\right)$.
34 30	39·68	80 12	2 55	+80 13	98·30	26 52
52 30	43·37	44 30	46 5	+44 17	2·37	4 55
53 30	44·33	39 54	57 19	+35 47	1·86	4 11
54 30	46·12	38 24	78 16	+20 28	1·36	3 58
55 30	48·13	41 50	101 47	−30 44	1·26	4 28
56 30	49·45	44 00	117 13	−42 49	1·64	4 50
57 30	50·42	51 00	128 34	−54 24	2·15	6 10
73 30	53·62	81 00	166 1	−81 15	26·76	28 55

This table verifies Laws I., V. and II.

The principal incidence derived from the fifth column is $54° 59'$, and the quantity of light in the plane of incidence is 1·58

times that in the perpendicular plane. We are now approaching the circular limit, or azimuth of incident polarization, at which the reflected light at the principal incidence will be circularly polarized.

Table VI.

Azimuth of polarizer $= 85° 45'$. Red sunlight.

Incidence.	Compensator.	Analyser.	$e'-e-180°$.	ϕ.	$\dfrac{a}{b}$.	$\mathrm{Tan}^{-1}\left(\dfrac{J}{I}\right)$.
54 30	46·05	43 26	77 27	+37 56	1·25	4 2
54 45	46·75	43 20	85 38	+26 18	1·09	4 1
55 0	46·90	43 8	87 24	+17 24	1·08	3 59
55 15	47·53	43 15	94 46	−26 49	1·11	4 0
55 30	48·05	45 30	100 51	−47 39	1·21	4 20

Here we observe the ratio $\dfrac{a}{b}$ approaching unity, while the axis major, as shown by the fifth column, still follows the Law V. Laws I., II., III., V. are verified by this table.

The principal incidence is $55° 6'$, and the ratio of quantities of light reflected in, and perpendicular to, the plane of incidence is $1·17$ at the principal incidence.

Table VII.

Azimuth of polarizer $= 85° 55'$. Red sunlight.

Incidence.	Compensator.	Analyser.	$e'-e-180°$.	ϕ.	$\dfrac{a}{b}$.	$\mathrm{Tan}^{-1}\left(\dfrac{J}{I}\right)$.
54 30	45·96	45 30	76 24	+47 7	1·27	4 10
54 45	46·65	45 12	84 28	÷47 4	1·10	4 7
55 0	47·00	45 5	88 34	+48 19	1·02	4 6
55 15	47·72	45 40	96 59	−50 25	1·13	4 11
55 30	48·00	46 30	100 16	−53 12	1·20	4 18

In this table we have just passed the circular limit, and the column for ϕ begins to show Law VI., which will be more fully developed in the succeeding experiments.

The principal incidence is $55° 7'$; and the minimum ratio of the quantities of light reflected in, and perpendicular to, the plane of incidence is $1·04$.

Table VIII.

Azimuth of polarizer = 86°. Red sunlight.

Incidence.	Compensator.	Analyser.	$e'-e-180°$.	ϕ.	$\frac{a}{b}$.	$\tan^{-1}\left(\frac{J}{I}\right)$.
34 30	39·77	83 20	3 58	+83 21	120·5	30 53
52 30	43·03	53 30	42 7	+56 16	2·73	5 24
53 30	44·67	47 0	61 18	+49 8	1·70	4 17
54 0	45·25	46 20	68 5	+48 34	1·48	4 11
54 30	46·13	46 11	78 23	+50 48	1·23	4 10
54 45	46·48	46 0	82 28	+52 28	1·14	4 8
55 0	46·91	45 45	87 30	+60 29	1·06	4 6
55 15	47·33	47 0	92 25	−74 27	1·08	4 17
55 30	48·07	48 20	101 5	−60 39	1·25	4 30
56 0	48·86	50 15	111 30	−58 25	1·53	4 48
56 30	49·40	51 30	116 38	−58 37	1·71	5 2
57 30	50·20	57 0	126 0	−63 34	2·26	6 9
73 30	53·69	83 15	166 50	−83 25	37·96	30 35

In this table the value of ϕ diminishes from 83° to 48°, and rises then to 60°; as the incidence approaches the principal incidence the motion becomes more rapid, and at that incidence the axis major is perpendicular to the plane of incidence: having passed this angle, it diminishes again to 58° negative, and then rises again to 83° negative. This motion of the axis major verifies Law VI., and explains the apparent anomaly in the value of ϕ in Table VII. A comparison of the values of $\frac{a}{b}$ in Tables VII. and VIII. proves the truth of Law IV.

The principal incidence is 55° 8′, at which angle, since the axis major is perpendicular to the plane of incidence, the quantity of light reflected in the plane of incidence is to the light reflected in the perpendicular plane as 1 : 1·12.

Table IX.

Azimuth of polarizer = 87°. Red sunlight.

Incidence.	Compensator.	Analyser.	$e'-e-180°$.	ϕ.	$\frac{a}{b}$.	$\tan^{-1}\left(\frac{J}{I}\right)$.
34 30	39·48	84 50	0 27	+84 50	Infinity.	30 6
52 30	42·87	60 0	40 14	+63 33	3·27	5 11
53 30	44·06	55 0	54 10	+60 56	2·16	4 17
54 0	44·93	54 20	64 21	+63 59	1·78	4 11
54 30	45·82	54 0	74 45	+70 30	1·52	4 8
54 45	46·34	53 54	80 50	+76 48	1·43	4 7
55 0	46·60	53 55	83 53	+80 50	1·40	4 7
55 15	47·23	53 34	91 15	−87 59	1·35	4 4
55 30	47·92	55 30	99 20	−78 33	1·48	4 22
56 0	48·40	56 30	104 57	−74 21	1·64	4 32
56 30	49·08	59 30	112 54	−72 28	1·97	5 5
57 30	50·00	64 30	123 40	−72 48	2·72	6 16
73 30	53·54	85 0	165 5	−85 10	44·5	30 55

An attentive examination of the fifth column proves completely the truth of Law VI.; and a comparison of the values of $\frac{a}{b}$ in Tables VII., VIII., IX. proves the truth of Law IV.

The principal incidence is 55° 13′, and the quantity of light in the plane of incidence is to the quantity of light in the perpendicular plane as 1 : 1·82.

Having ascertained the truth of the six laws stated at the commencement of this paper, I removed the compensator and made the following direct experiment, by which is shown the possibility of obtaining circularly-polarized light by means of a single reflexion, not total, at the surface of a transparent body :—

Angle of incidence . . =55° 0′
Polarizer . . . . . =85° 50′.

On turning the analyser round through 360°, it was not possible to detect any change in the intensity of the light, which was therefore sensibly circularly polarized, proving the truth of Law III. directly.

The truth of Laws II., III., IV. is made very evident by the following table formed from the preceding tables.

Table X.

Polarizer. $\left(\frac{a^2}{b^2}\right)$	Principal incidence.						
	45° 0′	80° 0′	85° 0′	85° 45′	85° 55′	86° 0′	87° 0′
	104·00	3·96	1·58	1·17	1·04	1·12	1·82

From this table it appears that the value of the azimuth, which I have called the circular limit, is 85° 52′.

Collecting together the values of the principal incidence, or incidence for which the axes of the reflected elliptically-polarized light lie in, and perpendicular to, the plane of incidence, we find

Table XI.

Polarizer.	Principal incidence.
45° 0′	55° 8′
80 0	54 27
85 0	54 59
85 45	55 6
85 55	55 7
86 0	55 8
87 0	55 13
Mean	55 1

This angle should, according to received ideas, coincide with the polarizing angle, or its tangent should give the refractive index; the tangent of 55° 1′ is 1·4290, which is very different from the refractive index found directly, which is 1·6229; but I have repeatedly observed the refractive index found by reflexion differ considerably from that found by refraction, and believe that the law of Brewster is only approximate.

In order to ascertain this point, I made the following experiment on another rhomb of glass having the same refractive index as that used in my former experiments.

Table XII.

Azimuth of polarizer = 86°.   Red sunlight.

Incidence.	Compensator.	Analyser.	$e'-e-180°$.	ϕ.	$\frac{a}{b}$.
34 30	39·70	83 20	3 8	+83 20	Infinity.
52 30	43·33	53 30	45 37	+56 48	2·53
53 30	44·70	50 0	61 39	+55 11	1·73
54 0	45·60	49 0	72 11	+57 20	1·41
54 30	46·30	47 30	80 22	+58 48	1·21
54 45	46·79	46 45	85 58	+65 30	1·10
55 0	47·31	46 0	92 11	−66 15	1·05
55 15	47·70	47 30	96 45	−63 20	1·16
55 30	48·00	48 30	100 16	−62 17	1·24
56 0	48·83	50 30	109 58	−59 50	1·50
56 30	49·10	52 30	113 8	−62 9	1·64
57 30	49·95	57 0	123 5	−64 32	2·15
73 30	53·40	82 30	163 27	−82 48	26·98

The refractive constants of this rhomb were determined with care, and found to be—

Refractive index of extreme red . . . . = 1·6190
Refractive index of extreme violet . . . = 1·6555
Refractive index of red used in experiments = 1·6230
Dispersive power . . . . = 0·0573

The comparison of the values of ϕ and $\frac{a}{b}$ from this table with the corresponding values in Table VIII. is satisfactory.

The principal incidence is 54° 53′, which agrees with the values in Table XI., and confirms the deduction I have drawn from that table.

The observations contained in the preceding tables were made during the course of the past summer, which was very favourable for such a purpose. I have repeated the more important observations many times, and by using strong sunlight have always succeeded in obtaining the same results. I consider that they

completely establish the six laws laid down by me; but I could have wished, before communicating them to your valuable Journal, to have had an opportunity of repeating them with different substances, particularly with double refracting crystals; but the short, rainy days of November, and the return of the college duties of Michaelmas term, compel me to postpone further experiments to a period of brighter sunshine and greater leisure. In the meantime, I think the facts I have already obtained will prove of some interest to such of your readers as are engaged in optical researches.

Trinity College, Dublin,
    November 4, 1854.

LXII. *The Theory of the Moon's Motion.—Second Approximation.*
    By Professor CHALLIS[*].

MR. ADAMS has made no reply to the arguments in the Philosophical Magazine for August, by which I met his objections to my new solution of the problem of the moon's motion. As those objections are all completely answered, I consider myself entitled to assert that the judgement passed on the paper which contained the first approximation of the solution remains unsupported. I have distinctly proved, that, in forming his opinion of it, Mr. Adams relied on false reasoning. I feel, therefore, no hesitation in extending the method to higher approximations, being well persuaded that such extension will more fully demonstrate its logical accuracy and the soundness of the deductions drawn from it. The object of the present communication is to obtain expressions for the radius-vector and true longitude of the moon to the second approximation, after recapitulating, for the sake of clearness, the reasoning of the first approximation.

It will be proper to begin with stating the limitations of the problem. Three bodies being supposed to attract each other according to the law of gravity, and the velocity and direction of the velocity of one of them at a given point of space being given at a given instant, it might be proposed to determine by successive approximations its subsequent motion on the supposition that the motion is principally due to the action of one of the other bodies. This, however, is not the problem relating to the moon's orbit which I propose to solve. I make the additional limitation, suggested by observation, that the moon's motion is such that it always differs to a small amount from uniform motion in a circular orbit of given radius. The problem

with this limitation is that of which the solution is usually called the lunar theory, although the introduction of the limitation into the reasoning is not usually pointed out.

Let M, m, m' be the attractions of the earth, moon, and sun respectively, at the unit of distance, and at the time t reckoned from a given epoch; let x, y, z, r and x', y', z', r' be the rectangular coordinates and radius-vectors of the moon and sun, referred to the earth's centre as a fixed origin, and to the plane of the ecliptic and the first point of Aries. Then putting μ for M $+ m$, and P for $(x-x')^2 + (y-y')^2 + (z-z')^2$, we have the known equations,

$$\frac{d^2x}{dt^2} + \frac{\mu x}{r^3} + \frac{m'x'}{r'^3} + m'(x-x')\mathrm{P}^{-\frac{3}{2}} = 0,$$

$$\frac{d^2y}{dt^2} + \frac{\mu y}{r^3} + \frac{m'y'}{r'^3} + m'(y-y')\mathrm{P}^{-\frac{3}{2}} = 0,$$

$$\frac{d^2z}{dt^2} + \frac{\mu z}{r^3} + \frac{m'z'}{r'^3} + m'(z-z')\mathrm{P}^{-\frac{3}{2}} = 0.$$

As it is not my object to obtain exact numerical results, but to exhibit a method of solution, I shall suppose for the sake of simplicity that the sun describes a circular orbit in the plane of the ecliptic at its mean distance (a'), and with its mean angular velocity (n'). Thus $x' = a' \cos (n't + \epsilon')$, $y' = a' \sin (n't + \epsilon')$, and $z' = 0$. Hence $dx' = -n'y'dt$, and $dy' = n'x'dt$. By taking account of these values of dx' and dy', and putting a' for r', the following result is readily obtained:

$$2dx \frac{d^2x}{dt^2} + 2dy \frac{d^2y}{dt^2} + 2dz \frac{d^2z}{dt^2} - 2n'x \frac{d^2y}{dt^2} + 2n'y \frac{d^2x}{dt^2} =$$

$$-\frac{2\mu}{r^2} dr - \frac{2m'}{a'^3} d.(xx'+yy') + 2m'd.(a'^2 - 2(xx'+yy') + r^2)^{-\frac{1}{2}}.$$

Hence by integration (putting ϕ for the angle between the radius-vectors of the sun and moon),

$$\frac{dx^2}{dt^2} + \frac{dy^2}{dt^2} + \frac{dz^2}{dt^2} - 2n'x \frac{dy}{dt} + 2n'y \frac{dx}{dt} + \mathrm{C}_1 =$$

$$\frac{2\mu}{r} - \frac{2m'r}{a'^2} \cos \phi + 2m'(a'^2 - 2a'r \cos \phi + r^2)^{-\frac{1}{2}}.$$

It thus appears that the *problem of three bodies admits of an exact first integral in the case in which the relative orbit of one of the bodies is a circle.* I am not aware that this proposition has been proved before.

To simplify the question still further, I shall now suppose the moon to move in the plane of the ecliptic, as the principle of the proposed method of solution equally admits of being exhibited

under this restriction. Thus we shall have, θ being the moon's longitude,

$$\frac{dz}{dt}=0, \quad \frac{dx^2}{dt^2}+\frac{dy^2}{dt^2}=\frac{dr^2}{dt^2}+\frac{r^2 d\theta^2}{dt^2},$$

$$r^2\frac{d\theta}{dt}=x\frac{dy}{dt}-y\frac{dx}{dt}, \quad \phi=\theta-(n't+\epsilon'),$$

and the above integral becomes

$$\left.\begin{array}{l}\dfrac{dr^2}{dt^2}+\dfrac{r^2 d\phi^2}{dt^2}+C_1=n'^2 r^2 \\[2mm] \qquad +\dfrac{2\mu}{r}-\dfrac{2m'r}{a'^2}\cos\phi+\dfrac{2m}{a'}\left(1-\dfrac{2r}{a'}\cos\phi+\dfrac{r^2}{a^2}\right)^{-\frac{1}{2}}\end{array}\right\} \quad \text{(a)}$$

Also, since

$$\frac{d\theta}{dt}=\frac{d\phi}{dt}+n' \text{ and } \frac{d\cdot\dfrac{r^2 d\theta}{dt}}{dt}=x\frac{d^2y}{dt^2}-y\frac{d^2x}{dt^2}$$

it follows that

$$\frac{d\cdot r^2\left(\dfrac{d\phi}{dt}+n'\right)}{dt}=\frac{m'r\sin\phi}{a'^2}\cdot\left\{1-\left(1-\frac{2r}{a'}\cos\phi+\frac{r^2}{a'^2}\right)^{-\frac{3}{2}}\right\}. \quad \text{(b)}$$

The equations (a) and (b) between the three variables r, ϕ and t, will conduct by successive approximations to the moon's motion and the form of her orbit. By expanding the trinomial affected with the indices $-\dfrac{1}{2}$ and $-\dfrac{3}{2}$ to the fourth power of $\dfrac{r}{a'}$, the following approximate equations, in which n'^2 is substituted for $\dfrac{m'}{a'^3}$, will be found:

$$\left.\begin{array}{l}\dfrac{dr^2}{dt^2}+r^2\dfrac{d\phi^2}{dt^2}+C_1-\dfrac{2m'}{a'}=\dfrac{2\mu}{r}+\dfrac{3n'^2 r^2}{2}(1+\cos2\phi) \\[3mm] \qquad +\dfrac{n'^2 r^3}{4a'}(3\cos\phi+5\cos3\phi) \\[3mm] \qquad +\dfrac{n'^2 r^4}{32a'^2}(9+20\cos2\phi+35\cos4\phi)\end{array}\right\} \text{(A)}$$

$$\left.\begin{array}{l}\dfrac{d\cdot r^2\left(\dfrac{d\phi}{dt}+n'\right)}{dt}=-\dfrac{3n'^2 r^2}{2}\sin2\phi \\[3mm] \qquad -\dfrac{3n'^2 r^3}{8a'}(\sin\phi+5\sin3\phi) \\[3mm] \qquad -\dfrac{5n'^2 r^4}{16a'^2}(2\sin2\phi+7\sin4\phi)\end{array}\right\} \cdot\cdot\cdot \text{(B)}$$

It may be remarked, that the method so far applies as well to the planetary as the lunar theory.

First Approximation.

Terms on the right-hand sides of the above equations involving higher powers of r than the second will be omitted. Thus the second equation may be put under the form

$$\frac{d \cdot r^2\left(\frac{d\phi}{dt} + n'\right)}{r^2\left(\frac{d\phi}{dt} + n'\right)} = -\frac{3n'^2 \sin 2\phi}{2\left(\frac{d\phi}{dt} + n'\right)} dt.$$

But by the hypothesis of the problem, the moon's longitude θ always differs by a small angle from a mean longitude $nt + \epsilon$. Hence $\frac{d\phi}{dt} + n' = n$ nearly, and $dt = \frac{d\phi}{n - n'}$. Substituting these values in the above equation, integrating, and omitting the square of the disturbing force, we have

$$r^2\left(\frac{d\phi}{dt} + n'\right) = h\left(1 + \frac{3n'^2 \cos 2\phi}{4n(n - n')}\right)$$

$$= h + \frac{3n'^2 r^2 \cos 2\phi}{4(n - n')} \text{ nearly.}$$

Hence

$$\frac{r d\phi}{dt} = \frac{h}{r} - n'r + \frac{3n'^2 r \cos 2\phi}{4(n - n')}$$

and

$$\frac{r^2 d\phi^2}{dt^2} = \frac{h^2}{r^2} - 2n'h + n'^2 r^2\left(1 + \frac{3 \cos 2\phi}{2}\right) \text{ nearly.}$$

But by the equation (A), to the same approximation,

$$\frac{dr^2}{dt^2} + \frac{r^2 d\phi^2}{dt^2} + C_1 - \frac{2m'}{a'} = \frac{2\mu}{r} + \frac{3n'^2 r^2}{2} \cos 2\phi.$$

Hence by substitution, and altering the designation of the arbitrary constant,

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{n'^2 r^2}{2} + C = 0. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (C)$$

It may be observed, that in obtaining this equation it has not been necessary to employ an approximate value of the radius-vector. It follows, however, from the reasoning, that r differs little from a mean value, because approximately

$$r^2\left(\frac{d\phi}{dt} + n'\right) = r^2 n = h.$$

If the equation (C) be supposed to apply to a circular orbit, we

have $\frac{dr}{dt} = 0$, and consequently

$$h^2 - 2\mu r + Cr^2 - \frac{n'^2 r^4}{2} = 0.$$

As this equation contains possible but not unequal roots, it is satisfied simultaneously with the derived equation

$$-\mu + Cr - n'^2 r^3 = 0.$$

Hence

$$r = \frac{\mu}{C} + \frac{n'^2 r^3}{C}$$

$$= \frac{\mu}{C} + \frac{n'^2 \mu^3}{C^4} \text{ nearly.}$$

This will afterwards be found to be the value of the mean distance in the first approximation to the orbit.

As the equation (C) gives the value of $\frac{dr^2}{dt^2}$ to the second order of small quantities, we may deduce from it the value of $\frac{dr}{dt}$, and therefore that of r, to the first order of small quantities; and these values, it may be observed, will be independent of the sun's longitude. I proceed next to the integration of that equation.

The course which first presents itself is, to substitute for r in the term involving n'^2 a mean value a, thus omitting, as heretofore, small quantities of the third order. By this process the approximate orbit is shown to be a fixed ellipse described by the action of the central force $\frac{\mu}{r^2}$. This result does not recognize the existence of a disturbing force, although it has been fully taken into account in the investigation. I find also, by carrying the approximation further on the same principle, that terms inconsistent with the hypothesis of the approximation arise, of the same kind as those met with in the usual method of treating the lunar theory when the approximation commences with a fixed ellipse. Since, however, no rule by which approximations are conducted has been violated, some step incompatible with the particular conditions of the question must have been taken. But the *only* step that has been taken peculiar to the case, is that of substituting a for r in the term $\frac{n'^2 r^2}{2}$, on the assumption that some other term of the equation is of a higher order of value than this term. It must therefore be concluded that the equation (C) contains no term of a higher order of value than that of the term $\frac{n'^2 r^2}{2}$. It is, however, certain that this is not the

case unless the trinomial $h^2 - 2\mu r + Cr^2$ be equivalent to a single term of the second order of small quantities. To satisfy this condition, the constants h and C must be related to each other. The required relation may be found as follows. Put for r in the above expression $w + v$, and assume w to be much larger than v. Then the expression becomes

$$h^2 - 2\mu(w + v) + C(w + v)^2,$$

and the appropriate condition is satisfied if

$$h^2 - 2\mu w + Cw^2 = 0$$

and

$$-2\mu + 2Cw = 0.$$

Hence it follows that

$$w = \frac{\mu}{C} \text{ and } h^2 = \frac{\mu^2}{C}.$$

The reasoning by which the above relation between h and C is deduced, appears to be perfectly cogent. I cannot, after the fullest consideration, perceive that any step can be called in question. It may be observed that the constant C was introduced by an integration performed anterior to any limitation of the question, and that the relation between h and C results from the limitation given to the problem by assuming the moon's true motion in longitude to differ little from a mean motion. It is not my intention to introduce the equation $h^2C = \mu^2$ into the investigation at present, because I wish to prove, first, that if h and C be regarded as independent of each other, the solution of the lunar problem deduced from the equation (C) is *identical* with the ordinary solution. It must, however, be borne in mind, that if the foregoing reasoning be good, neither method of solution ought, in strict logic, to be proceeded with until the above relation between the constants has been deduced[*].

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[*] As in this part of the reasoning my method is distinguished from every other that has been applied to the lunar theory, I will endeavour to put the argument in as succinct a form as possible, that it may be the more readily seized.

Suppose the expression $-Cr^2 - h^2 + 2\mu r + \dfrac{n'^2 r^4}{2}$ to contain terms higher in value than the last term.

Then the *only* legitimate process of approximation is to integrate, neglecting the small term, so as to obtain an approximate value of r, to substitute this value in the small term and integrate again, and so on.

By this process the approximation commences with a fixed ellipse of arbitrary eccentricity, and is found in succeeding steps to introduce terms which may increase indefinitely with the time, and which are therefore incompatible with the hypothesis that the true values of the radius-vector and the longitude differ little from mean values.

Consequently on that hypothesis the above expression cannot contain

Putting now the equation (C) under the form

$$dt = \frac{r\,dr}{\sqrt{-Cr^2 - h^2 + 2\mu r + \frac{n'^2 r^4}{2}}},$$

it follows from what is shown above, that the integration must be performed as if the quantity under the radical were of *four* dimensions with respect to r, the last term being treated as variable. This cannot be done exactly, but an integral sufficiently approximate for our purpose may be obtained on the same principle as that applied to the approximate solution of algebraic equations of high dimensions. That is, having ascertained in the manner exhibited above that $\frac{\mu}{C}$ is an approximate value of r, we may substitute in the above equation $\frac{\mu}{C} + v$ for r, and expand to the *second* power of v, to secure an approximation of the first order. This being done, the equation is integrable, and the same results are obtained as those given in my communication to the Philosophical Magazine for April 1854 (p. 281). It will only be necessary to insert here those results which may be useful in the second approximation.

$$c(nt + \epsilon + \gamma) = \cos^{-1}\frac{a-r}{ae} - \left(e^2 - \left(1 - \frac{r}{a}\right)^2\right)^{\frac{1}{2}},$$

$$\frac{a}{r} = 1 + e \cos c(\theta + \gamma)$$

$$c = 1 - \frac{3m^2}{4}, \quad n = \frac{\sqrt{\mu}}{a^{\frac{3}{2}}}\left(1 - \frac{m^2}{4}\right),$$

m being the ratio of the moon's periodic time to the sun's, and a and e being new constants, the relations of which to the constants h and C are given by the equations

$$a = \frac{\mu}{C}(1 + m^2), \quad e^2 = 1 - \frac{h^2 C}{\mu^2} + \frac{m^2}{2}.$$

From the last two equations may be deduced the following:

$$C = \frac{\mu}{a}(1 + m^2), \quad h^2 = \mu a\left(1 - e^2 - \frac{m^2}{2}\right).$$

Hence

$$h = na^2\left(1 - \frac{e^2}{2}\right).$$

terms higher in value than the last term, and the constants C and h must be related to each other in such a manner as to satisfy this condition.

The relation between C and h thus obtained is the same that results by supposing the expression to contain the disturbing force as a *factor*.

The first approximations to the radius-vector and the true longitude in terms of the mean longitude as given by this solution, are

$$r = a - ae \cos c . \overline{nt + \epsilon + \gamma}$$
$$\theta = nt + \epsilon + 2e \sin c . \overline{nt + \epsilon + \gamma}.$$

Second Approximation.

The same terms of the equations (A) and (B) are to be used for the second approximation as for the first, the next terms being of a higher order by two degrees. Thus the equation (B) gives

$$d . \left\{ r^2 \left(\frac{d\phi}{dt} + n' \right) \right\} = -\frac{3}{2} n'^2 r^2 \sin 2\phi \, dt.$$

Putting, for shortness' sake, p for the angle $c(nt + \epsilon + \gamma)$, and q for $(nt + \epsilon) - (n't + \epsilon')$, and using the values of r and θ given by the first approximation, it will be found that

$$r^2 \sin 2\phi = a^2 \big(\sin 2q + e \sin (2q + p) - 3e \sin (2q - p) \big).$$

Substituting in the equation above, integrating, and omitting terms of the fourth order,

$$r^2 \left(\frac{d\phi}{dt} + n' \right) = h + \frac{3n'^2 a^2}{2n} \left(\frac{1}{2} \cos 2q + \frac{e}{3} \cos (2q + p) - 3e \cos (2q - p) \right).$$

Hence by squaring, and substituting the approximate value of r in the small term,

$$r^2 \frac{d\phi^2}{dt^2} = \frac{h^2}{r^2} - 2n'h + n'^2 r^2$$
$$+ \frac{3n'^2 a^2}{2} \left(\cos 2q + \frac{5e}{3} \cos (2q + p) - 5e \cos (2q - p) \right).$$

Again, by a like process,

$$r^2 \cos 2\phi = a^2 \big(\cos 2q - 3e \cos (2q - p) + e \cos (2q + p) \big).$$

Hence the equation (A) gives

$$\frac{dr^2}{dt^2} + r^2 \frac{d\phi^2}{dt^2} = C_1 + \frac{2m'}{a'} + \frac{2\mu}{r} + \frac{3n'^2 r^2}{2}$$
$$+ \frac{3n'^2 a^2}{2} \big(\cos 2q - 3e \cos (2q - p) + e \cos (2q + p) \big).$$

Consequently, by subtraction,

$$\frac{dr^2}{dt^2} + \frac{h^2}{r^2} - \frac{2\mu}{r} - \frac{n'^2 r^2}{2} + C = n'^2 a^2 e \big(3 \cos (2q - p) - \cos (2q + p) \big). \quad (C')$$

It will be seen that this approximation has introduced no new term independent of the sun's longitude. Putting, for brevity,

Q for $-\dfrac{h^2}{r^2} + \dfrac{2\mu}{r} + \dfrac{n'^2 r^2}{2} - C$, the approximate value of dt may be thus expressed :

$$dt = \frac{dr}{\sqrt{Q}} - \frac{n'^2 a^2 e}{2 Q^2}\Big(3 \cos (2q-p) - \cos (2q+p)\Big) dt.$$

The first term of the right-hand side of the equation is to be integrated just as in the first approximation. In the other term we may substitute for Q^2 the approximate value of $\dfrac{dr^2}{dt^2}$, viz. $n^2 a^2 e^2 \sin^2 p$. Then observing that

$$\int \frac{3 \cos (2q-p) - \cos (2q+p)}{\sin^2 p} dt = -\frac{2 \cos 2q}{n \sin p} \text{ nearly,}$$

we obtain by integration,

$$c(nt + \epsilon + \gamma) = \cos^{-1} \frac{a-r}{ae} - \frac{1}{a} \sqrt{a^2 e^2 - (a-r)^2} + \frac{n'^2}{n^2 e} \cdot \frac{\cos 2q}{\sin p}.$$

But $\sqrt{a^2 e^2 - (a-r)^2} = ae \sin p$ nearly, and $\dfrac{n'^2}{n^2} = m^2$. Hence it will be readily seen that

$$r = a\left(1 - e \cos p + \frac{e^2}{2} - \frac{e^2}{2} \cos 2p - m^2 \cos 2q\right).$$

Again, by what is proved above, we have to quantities of the second order,

$$\frac{d\theta}{dt} = \frac{h}{r^2} + \frac{3n'^2}{4n} \cos 2q.$$

Hence

$$\theta = \int \frac{h\,dt}{r^2} + \frac{3m^2}{8} \sin 2q.$$

Putting for r the value just obtained, and for h its value $na^2\left(1 - \dfrac{e^2}{2}\right)$, and integrating, the result is

$$\theta = nt + \epsilon + 2e \sin p + \frac{5e^2}{4} \sin 2p + \frac{11m^2}{8} \sin 2q.$$

We have thus arrived at the values of r and θ given by the second approximation. But it does not follow that these are the complete values to the second order of small quantities; because, on proceeding to the next approximation, terms of the third order may rise to the second order by integration. This will be the case with terms of which the circular function contains the arc $2(q-p)$. I propose, therefore, to enter upon the *third* approximation so far as may be necessary to discover terms which rise to the second order by integration. For this purpose

the values of r and θ given by the second approximation will be employed.

On going through the process exactly in the manner already indicated, it is found that the expression for $\dfrac{dr^2}{dt^2}$ contains the term

$$3n'^2a^2 \cdot \frac{5e^2}{4m} \cos 2(q-p),$$

which on account of the small denominator m is of the third order. This gives rise in the value of r, to the term

$$-\frac{n'^2}{2n^2e} \cdot \int \frac{15e^2}{4m} \cdot \frac{\cos 2(q-p)}{\sin^2 p}\, dt,$$

or

$$-\frac{15me}{8} \int \frac{\cos 2(q-p)}{\sin^2 p}\, dt.$$

But

$$\int \frac{\cos 2(q-p)}{\sin^2 p}\, dt = -\frac{\cos(2q-p)}{n \sin p} \text{ nearly.}$$

Hence the additional term in the value of r is

$$-\frac{15ame}{8} \cos(2q-p),$$

and we have, inclusive of all terms of the second order,

$$\frac{r}{a} = 1 - e\cos p + \frac{e^2}{2} - \frac{e^2}{2}\cos 2p - m^2 \cos 2q - \frac{15me}{8}\cos(2q-p).$$

This value, substituted in the equation

$$\theta = \int \frac{hdt}{r^2} + \frac{3m^2}{8} \sin 2q,$$

gives

$$\theta = nt + \epsilon + 2e\sin p + \frac{5e^2}{4}\sin 2p + \frac{11m^2}{8}\sin 2q + \frac{15me}{4}\sin(2q-p).$$

We have thus arrived at the well-known expressions for the radius-vector and the longitude to small quantities of the second order. Thus my equation (C), which was pronounced to be unfit for giving accurate information, has given in a very direct manner precisely the same information as the most approved methods, and has added other information, not previously obtained, which I believe to be equally trustworthy.

I purpose to apply the same method to the third, and possibly the fourth approximation, as soon as I can get leisure for the large amount of calculation which this investigation will require.

Cambridge Observatory,
    October 20, 1854.

LXIII. *On Formulæ for the Maximum Pressure and Latent Heat of Vapours.* By W. J. Macquorn Rankine, *Civil Engineer,* F.R.SS. *Lond. and Edinb.* &c.*

1. IT is natural to regard the pressure which a liquid or solid and its vapour maintain when in contact with each other and *in equilibrio,* as the result of an expansive elasticity in the vapour, balanced by an attractive force which tends to condense it on the surface of the liquid or solid, and which is very intense at that surface, but inappreciable at all sensible distances from it. According to this view, every solid or liquid substance is enveloped by an atmosphere of its own vapour, whose density close to the surface is very great, and diminishes at first very rapidly in receding from the surface; but at appreciable distances from the surface is sensibly uniform, being a function of the temperature and of the attractive force in question.

2. Many years since I investigated mathematically the consequences of this supposition, and arrived at the conclusion, that although it is impossible to deduce from it, in the existing condition of our knowledge of the laws of molecular forces, the exact nature of the relation between the temperature and the maximum pressure of a vapour, yet that if the hypothesis be true, it is probable that an approximate formula for the logarithm of that pressure for any substance will be found, by subtracting from a constant quantity, a converging series in terms of the powers of the reciprocal of the absolute temperature, the constant and the coefficients of the series being determined for each substance from experimental data. Such a formula is represented by

$$\log P = A - \frac{B}{\tau} - \frac{C}{\tau^2} - \&c.,$$

where P denotes the pressure, τ the absolute temperature, that is, the temperature as measured from the absolute zero of a perfect gas thermometer, A the constant term, and B, C, &c. the coefficients of the converging series.

3. On applying this formula to M. Regnault's experiments on the pressure of steam, it was found that the first three terms were sufficient to represent the results of these experiments with minute accuracy throughout their whole extent; that is to say, between the temperatures of

$$-30° \text{ and } + 230° \text{ Centigrade}$$
$$= -22° \text{ and } \quad 446° \text{ Fahrenheit,}$$

and between the pressures of $\frac{1}{2200}$th of an atmosphere, and 82 atmospheres.

* Communicated by the Author; having been read to the British Association for the Advancement of Science, Section A, at Liverpool, Sept. 1854.

Formulæ of three terms were also found to represent the results of Dr. Ure's experiments on the vapours of alcohol and æther, and formulæ of two terms, those of his experiments on the vapours of turpentine and petroleum, as closely as could be expected from the degree of precision of the experiments. A formula of two terms was found to represent accurately the results of M. Regnault's experiments on the vapour of mercury.

4. These formulæ, with a comparison between their results and those of the experiments referred to, were published in the Edinburgh New Philosophical Journal for July 1849, in a paper the substance of which is summed up at its conclusion in the following proposition :—

If the maximum elasticity of any vapour in contact with its liquid be ascertained for three points on the scale of the air-thermometer, then the constants of an equation of the form

$$\log P = A - \frac{B}{\tau} - \frac{C}{\tau^2}$$

may be determined, which equation will give, for that vapour, with an accuracy limited only by the errors of observation, the relation between the temperature (τ), measured from the absolute zero, and the maximum elasticity (P), at all temperatures between those three points, and for a considerable range beyond them.

5. In the case of water and mercury, the precision of the experimental data left nothing to be desired. I have, however, in the table of constants at the end of this paper, so far modified the coefficients for water and mercury as to adapt them to a position of the absolute zero (274° Centigrade, or 493°·2 Fahrenheit below the temperature of melting ice), which is probably nearer the truth than that employed in the original paper, which was six-tenths of a Centigrade degree lower. This modification, however, produces no practically appreciable alteration in the numerical results of the formulæ.

6. It was otherwise with respect to the other fluids mentioned, for which the experimental data were deficient in precision, so that the values of the constants could only be regarded as provisional.

7. A summary, published in the *Comptes Rendus* for the 14th of August 1854 *, of the extensive and accurate experiments of M. Regnault on the elasticities of the vapours of æther, sulphuret of carbon, alcohol, chloroform, and essence of turpentine, has now supplied the means of obtaining formulæ, founded on data as precise as it is at present practicable to obtain, for the maximum pressures of these vapours.

A synopsis of these formulæ, and of the constants contained in them, is annexed to this paper. The constants, as given in

* See p. 269 of the present volume of this Journal.

the table, are suited for millimetres of mercury as the measure of pressures, and for the scale of the Centigrade thermometer; but logarithms are given, by adding which to them they can be easily adapted to other scales.

The limited time which has elapsed since the publication of M. Regnault's experiments prevents my being yet able to bring the details of the investigation, of the formulæ, and of the comparison of their results with those of experiment, into a shape suited for publication; but I shall here add some remarks on their degree of accuracy and the extent of their applicability.

8. M. Regnault explains, that his experiments were made by two methods; at low temperatures, by determining the pressure of the vapour *in vacuo*; at high temperatures, by determining the boiling-point under the pressure of an artificial atmosphere. For each fluid, the pressures determined by both those methods were compared throughout a certain series of intermediate temperatures.

For all fluids in a state of absolute purity, the results of those two methods agreed exactly (as M. Regnault had previously shown to be the case for water).

The presence, however, of a very minute quantity of a foreign substance in the liquid under experiment was sufficient to make the pressure of the vapour *in vacuo* considerably greater than the pressure of ebullition at a given temperature; and it would appear, also, that a slight degree of impurity affects the accuracy even of the latter method of observation, although by far the more accurate of the two when they disagree.

9. The degree of precision with which it has been found possible to represent the results of the experiments by means of the formulæ, corresponds in a remarkable manner with the degree of purity in which, according to M. Regnault, the liquid can be obtained.

Sulphuret of Carbon, M. Regnault states, can easily be obtained perfectly pure. For this fluid, the agreement of the pressures computed by the formula with those determined by experiment throughout the whole range of temperature from −16° Cent. to +136°, is almost as close as in the case of steam.

Æther and *Alcohol* are less easy to be obtained perfectly pure. The discrepancies between calculation and experiment in these cases, though still small, are greater than for sulphuret of carbon.

For æther the formula may be considered as practically correct throughout the whole range of the experiments, from −20° Centigrade to +116°; but for alcohol below 0° Centigrade, the discrepancies, though absolutely small quantities, are large relatively to the entire pressures; and the formula can be considered applicable above this temperature only.

Essence of Turpentine has been discovered by M. Regnault to undergo a molecular change by continued boiling. For this fluid the agreement between the formula and the experiments is satisfactory above 40° Centigrade, and up to the limit of the experiments, 222°, but not below 40°.

It is impossible to obtain *Chloroform* free from an admixture of foreign substances. Accordingly, M. Regnault has found that the two methods of determining the pressure of the vapour of this fluid give widely different results, neither of which can be represented accurately by the formula now proposed below the temperature of 70° Cent. From this temperature, however, up to 130° Cent., the limit of the experiments, the agreement is close.

10. In the cases of alcohol and turpentine, the discrepancies between the formulæ and the experiments at very low temperatures are such as to indicate that they might be removed by introducing a fourth term into the formulæ, inversely proportional to the cube of the absolute temperature; but the trifling and uncertain advantage to be thus obtained would be outweighed by the inconvenience in calculation, and especially by the necessity for solving a cubic equation in computing the temperature from the pressure; whereas with formulæ of three terms, it is only necessary to extract a square root, as the formula No. 2 shows.

11. Although, for the mere determination of the maximum pressure of a vapour at a given temperature, or its temperature at a given pressure, a table, or a curve drawn on a diagram may be sufficient, still there are many questions of thermo-dynamics respecting vapours for the solution of which a formula is essential.

Amongst these is the computation of the latent heat of evaporation, which is equivalent to the potential energy or work exerted by the vapour in overcoming external pressure, added to that exerted in overcoming molecular attraction. For *unity of weight* of a given substance, this is a function of the pressure, temperature, and density; but for a quantity of the substance such that its volume when evaporated exceeds its volume in the liquid or solid state by unity of cubic space, the latent heat of evaporation is simply the differential coefficient of the pressure with respect to the hyperbolic logarithm of the absolute temperature, as shown in the formula No. 3; so that, although the densities of the vapours of the seven fluids referred to in this paper are yet known by conjecture only, and not by direct experiment, we can, from the relation between the pressure and the temperature, determine accurately how much heat must be expended in the evaporation of so much of each of them as is necessary in order to propel a piston through a given space under a given *constant* pressure,

and thus to solve many problems connected with engines driven by vapours of different kinds.

12. It is somewhat remarkable, that the coefficients of the reciprocal of the temperature (B) in the formulæ for æther, sulphuret of carbon and alcohol, are nearly equal; as also those of the square of the reciprocal of the temperature (C) for æther and sulphuret of carbon.

In consequence of this, the pressure of the vapour of æther, and its latent heat for unity of space as above defined, at a given temperature, exceed the corresponding quantities for sulphuret of carbon at the same temperature, in a ratio which is nearly, though not exactly constant, and whose average value is somewhat less than 1·5.

Synopsis of the Formulæ, &c.

Notation.

τ = absolute temperature = temp. Centig. + 274° C.
$\qquad\qquad\qquad\qquad$ = temp. Fahr. + 461°·2 F.

P = maximum pressure of vapour at the absolute temperature τ.

v = volume of unity of weight of the liquid.

V = volume of unity of weight of saturated vapour.

L = latent heat of evaporation of unity of weight of the fluid expressed in units of work.

A, B, C constants.

Formulæ.

1. To find the maximum pressure from the temperature,

$$\text{com. } \log P = A - \frac{B}{\tau} - \frac{C}{\tau^2}.$$

2. To find the temperature from the maximum pressure,

$$\frac{1}{\tau} = \sqrt{\left\{ \frac{A - \text{com.} \log P}{C} + \frac{B^2}{4C^2} \right\}} - \frac{B}{2C}.$$

3. To find the latent heat of evaporation (expressed in units of work) of so much of the fluid that its bulk when evaporated exceeds its bulk in the liquid state by an unit of space, that is to say, of the weight $\dfrac{1}{V-v}$ of fluid. In this formula the pressure must be expressed in units of weight per square unit.

$$\frac{L}{V-v} = \tau \frac{dP}{d\tau} = P\left(\frac{B}{\tau} + \frac{2C}{\tau^2}\right) \times \text{hyp. log } 10.$$

(Hyp. log 10 = 2·30258509,
the common logarithm of which is 0·3622157.)

Units of work are reduced to units of heat (degrees in unity

of weight of liquid water) by dividing by Joule's equivalent of the specific heat of liquid water, which has the following values, according to the units of temperature and length employed.

		Logarithms.
Centigrade scale, and metres . .	423·54	2·6268969
Centigrade scale, and feet . .	1389·6	3·1428898
Fahrenheit's scale, and feet . .	772·0	2·8876173

Constants in the Formulæ for Pressures in millimetres of Mercury, and Temperatures in Centigrade degrees.

Fluids.	A.	Log B.	Log C.	$\frac{B}{2C}$.	$\frac{B^2}{4C^2}$.
Æther	7·1284	3·0596504	4·7065130	0·011275	0·00012712
Sulphuret of carbon ...	6·8990	3·0520049	4·7078426	0·011044	0·00012197
Alcohol above 0° C. ...	7·5259	3·0570610	5·2426805	0·0032610	0·000010634
Water	7·8143	3·1811430	5·0881857	0·0061934	0·000038358
Essence of turpentine } above 40° C.	6·2522	2·9625209	5·3712157	0·0019511	0·0000038067
Chloroform above 70° C...	5·8075	2·4007279	5·3919420	0·00051022	0·00000026032
Mercury up to 358° C...	7·5243	3·4675637			

To adapt the formulæ to other scales of pressure, add the following logarithms to the constants A :—

For inches of mercury	$\overline{2}$·59517
For kilogrammes on the square metre . .	1·13341
For pounds avoirdupois on the square foot .	0·44477

To adapt the formulæ to the scale of Fahrenheit's thermometer, multiply B by 1·8, and C by $(1·8)^2 = 3·24$; that is to say,

Add to log B . . .	0·2552725
Add to log C . . .	0·5105450.

LXIV. *Note on* Burman's *Law for the Inversion of the Independent Variable.* By J. J. SYLVESTER, *F.R.S.*[*]

THIS Note refers to the development of the *n*th differential coefficient of *u* in respect to *x* in terms of the *n*th and lower differential coefficients of *x* in respect to *u*.

The late Mr. Gregory, in his very valuable book of examples on the Calculus, in alluding to this development, speaks of it as "extremely complicated, and involving so much preliminary matter for its demonstration," that he contents himself "with referring to a memoir by Mr. Murphy on the subject in the Philosophical Transactions, 1837, p. 210." The development there given is of course essentially no other than that included in Bur-

man's general formula. I recently have had occasion (as a preliminary step to the investigation of the laws of inverse transformation between two systems of t variables each, instead of between two single variables only, an investigation in which I have already made such progress that I expect shortly to be in possession of the general formula for the purpose) to reconsider what I shall term Burman's law, and have been somewhat surprised to find, that so far from affording a complicated expression, it does, when properly stated, give rise to an expression of the very simplest form that could be conceived or desired, and one that admits of an easy and elementary proof.

To fix the ideas, let us take the case of $\dfrac{d^7u}{dx^7}$, where $x = \phi u$. For greater brevity write $\dfrac{d^r x}{du^r}$ as x_r. The most cursory consideration will suffice to show, irrespective of all calculation, that we should have the following form of expansion, viz.

$$\frac{d^7u}{dx^7} = -x_7 \div x_1^8$$

$$+ \{(2, 6)x_2 \cdot x_6 + (3, 5) \cdot (x_3 \cdot x_5) + (4, 4) \cdot (x_4 \cdot x_4)\} \div x_1^9$$

$$- \{(2,2,5)x_2 \cdot x_2 \cdot x_5 + (2,3,4) \cdot (x_2 \cdot x_3 \cdot x_4) + (3,3,3)(x_3 \cdot x_3 \cdot x_3) \div x_1^{10}$$

$$+ \{(2, 2, 2, 4)(x_2 \cdot x_2 \cdot x_2 \cdot x_4) + (2, 2, 3, 3)(x_2 \cdot x_2 \cdot x_3 \cdot x_3)\} \div x_1^{11}$$

$$- \{(2, 2, 2, 2, 3)(x_2 \cdot x_2 \cdot x_2 \cdot x_2 \cdot x_5) \div x_1^{12}\}$$

$$+ (2, 2, 2, 2, 2, 2) \div x_1^{13}.$$

In the first group of a single term, 7 is taken in one part, in the second group of 3 terms, 8 is taken in every possible way of partition in two parts, in the third group of 3 terms, 9 is taken in every possible way of partition in three parts, and so on, until finally 12, *i. e.* the double of the number next inferior to the given index 7, is taken in the sole possible way in which it can be taken of six parts; I ought to add, that in the groups of indices, *unity* is always understood to be inadmissible.

The groups of indices in the parentheses indicate numerical coefficients to be determined, and the whole and sole real difficulty (if any) of the question consists in determining the value of these numerical symbols. Now the law which furnishes these values would be seen on the most perfunctory examination to be the very simplest law that could possibly be stated, viz. any such symbol as $(r, s, t, \dots)$ is to be understood to denote the number of distinct ways in which a number of things equal to the sum of the indices r, s, t, &c. admit of being thrown into combination groups of r, s, t, &c. !

Thus, *ex. gr.*

$$(2, 6) = \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8}{1 \cdot 2 \times 1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6} = 28$$

$$(3, 5) = \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8}{1 \cdot 2 \cdot 3 \times 1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} = 56$$

$$(4, 4) = \frac{1}{2} \cdot \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8}{1 \cdot 2 \cdot 3 \cdot 4 \times 1 \cdot 2 \cdot 3 \cdot 4} = 35$$

$$(2,2,5) = \frac{1}{2} \cdot \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9}{1 \cdot 2 \times 1 \cdot 2 \times 1 \cdot 2 \cdot 3 \cdot 4 \cdot 5}$$

$$(2,3,4) = \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9}{1 \cdot 2 \times 1 \cdot 2 \cdot 3 \times 1 \cdot 2 \cdot 3 \cdot 4}$$

$$(3,3,3) = \frac{1}{1 \cdot 2 \cdot 3} \cdot \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9}{1 \cdot 2 \cdot 3 \times 1 \cdot 2 \cdot 3 \times 1 \cdot 2 \cdot 3}$$

$$(2,2,2,4) = \frac{1}{1 \cdot 2 \cdot 3} \cdot \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9 \cdot 10}{1 \cdot 2 \times 1 \cdot 2 \times 1 \cdot 2 \times 1 \cdot 2 \cdot 3 \cdot 4}$$

$$(2, 2, 3, 3) = \frac{1}{1 \cdot 2} \times \frac{1}{1 \cdot 2} \times \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9 \cdot 10}{1 \cdot 2 \times 1 \cdot 2 \times 1 \cdot 2 \cdot 3 \times 1 \cdot 2 \cdot 3},$$

and so on. The general law is obvious; and to prove its applicability in general, we have only to show that if it be true for the case of $\dfrac{d^r \cdot u}{dx^r}$, it is true for $\dfrac{d^{r+1} \cdot u}{dx^{r+1}}$. The proof is as follows. Let in general $[l, m, n, \&c.]$ indicate the value of

$$\frac{1 \cdot 2 \cdot 3 \cdot \cdot \cdot \cdot \cdot (l+m+n+\&c.)}{1 \cdot 2 \cdot \cdot \cdot l \times 1 \cdot 2 \cdot \cdot \cdot m \times 1 \cdot 2 \cdot \cdot \cdot n \times \&c.},$$

without reference to $l, m, n,$ &c. being equal or unequal *inter se.*

　Lemma 1. It is very easily seen that

$$[l, m, n, \&c.] = [l-1, m, n, \&c.] + [l, m-1, n, \&c.]$$
$$+ [l, m, n-1, \&c.] + \&c.$$

If now we use the notation $[r:\rho, s:\sigma, t:\tau, \&c.]$ as an abbreviated form of the notation $[\rho, \rho, \rho \ldots \text{to } r \text{ terms}, \sigma, \sigma, \sigma \ldots \text{to } s \text{ terms}, \tau, \tau \ldots \text{to } t \text{ terms}, \&c.]$, it is obvious that the equation last written becomes

$$[r:\rho, s:\sigma, t:\tau, \&c.] = r \times [\rho-1, (r-1):\rho, s:\sigma, t:\tau, \&c.]$$
$$+ s[r:\rho, \sigma-1, (s-1):\sigma, t:\tau, \&c.] + t[r:\rho, s:\sigma, \tau-1, (t-1)\tau, \&c.] + \&c.$$

　Lemma 2. Let $\mathrm{C}(r:\rho, s:\sigma, t:\tau, \&c.)$ denote the number of ways in which $r \cdot \rho + s \cdot \sigma + t \cdot \tau$, &c. can be taken in combinations of $\rho, \rho, \ldots$ to r places, $\sigma, \sigma, \ldots$ to s places, &c., then upon the supposition that $\rho, \sigma, \tau,$ &c., which are to be understood as arranged in an ascending order of magnitude, are all unequal, we shall have (using for shortness $\mathrm{G}(r)$ to denote $1 \cdot 2 \cdot 3 \ldots r$),

$$\mathrm{C}(r:\rho,\, s:\sigma,\, t:\tau,\, \&\mathrm{c}.)$$

$$=\frac{1}{\mathrm{G}(r)}\cdot\frac{1}{\mathrm{G}(s)}\cdot\frac{1}{\mathrm{G}(t)}\&\mathrm{c}.\times[r:\rho,\, s:\sigma,\, t:\tau,\, \&\mathrm{c}.],$$

which by Lemma 1

$$=\frac{1}{\mathrm{G}(r-1)}\cdot\frac{1}{\mathrm{G}(s)}\cdot\frac{1}{\mathrm{G}(t)}\times[\rho-1,\,(r-1):\rho,\, s:\sigma,\, t:\tau,\, \&\mathrm{c}.]$$

$$+\frac{1}{\mathrm{G}(r)}\cdot\frac{1}{\mathrm{G}(s-1)}\cdot\frac{1}{\mathrm{G}(t)}\times[r:\rho,\,\sigma-1,\,(s-1):\sigma,\, t:\tau,\, \&\mathrm{c}.]$$

$$+\frac{1}{\mathrm{G}(r)}\cdot\frac{1}{\mathrm{G}(s)}\cdot\frac{1}{\mathrm{G}(t-1)}\times[r:\rho,\, s:\sigma,\,\tau-1,\,(t-1)\tau,\, \&\mathrm{c}.]$$

$$+\&\mathrm{c}.$$

$$\left.\begin{aligned}
&=\mathrm{C}\big((\rho-1),\,(r-1):\rho,\, s:\sigma,\, t:\tau,\, \&\mathrm{c}.\big)\\
&+\{1+r\mathrm{F}(\sigma-\rho)\}\mathrm{C}(r:\rho,\,\sigma-1,\,(s-1):\sigma,\, t:\tau,\, \&\mathrm{c}.)\\
&+\{1+r\mathrm{F}(\tau-\sigma)\}\mathrm{C}(r:\rho,\, s:\sigma,\,\tau-1,\,(t-1)\tau,\, \&\mathrm{c}.)\\
&+\&\mathrm{c}.\ \&\mathrm{c}.
\end{aligned}\right\}$$

$\mathrm{F}(\sigma-\rho)$, $\mathrm{F}(\tau-\sigma)$, &c. meaning quantities which are respectively zero when $\sigma-1 > \rho$, $\tau-1 > \sigma$, &c., and respectively units when $(\sigma-1)=\rho$, $\tau-1=\sigma$, &c.; for it will be obvious that if $\sigma-1=\rho$, the quantity $[r:\rho,\,\sigma-1,\,(s-1):\sigma,\, t:\tau,\, \&\mathrm{c}.]$ becomes

$$[(r+1):\rho,\,(s-1):\sigma,\, t:\tau,\, \&\mathrm{c}.],$$

and consequently when divided by $\mathrm{G}(r)\,.\,\mathrm{G}(s)\,.\,\mathrm{G}(t)$, &c., does not give $\mathrm{C}\big((r+1):\rho,\,(s-1):\sigma,\, t:\tau,\, \&\mathrm{c}.\big)$, but

$$(1+r)\times\mathrm{C}\big((r+1):\rho,\,(s-1):\sigma,\, t:\tau,\, \&\mathrm{c}.\big),$$

and so similarly for the cases of $\tau-1=\sigma$, &c.

Now let us suppose that we are considering any group $(\rho\rho\ldots$ to r places, $\sigma\sigma\ldots$ to s places, &c.), or more briefly $(r:\rho,\, s:\sigma,\, t:\tau\ldots)$, the numerical coefficient of the term $x_\rho^r\,.\,x_\sigma^s\,.\,x_\tau^t\ldots$ in the inverse development of $\dfrac{d^\mu x}{dw^\mu}$.

And first, suppose that ρ is not 2.

The coefficient in question will evidently be made up exclusively of the following parts (each, however, affected with the factor $(-)^{\mathrm{N}-1}$) derived from the expansion of $\dfrac{d^{\mu-1}.x}{dw^{\mu-1}}$, for which the law to be established is supposed to hold, viz.

$$\left.\begin{aligned}
&\mathrm{C}\big(\rho-1,\,(r-1):\rho,\, s:\sigma,\, t:\tau,\, \&\mathrm{c}.\big)\\
&+\big(1+r\mathrm{F}(\sigma-\rho)\big)\mathrm{C}\big(r:\rho,\,\sigma-1,\,(s-1):\sigma,\, t:\tau,\, \&\mathrm{c}.\big)\\
&+\big(1+r\mathrm{F}(\tau-\sigma)\big)\mathrm{C}\big(r:\rho,\, s:\sigma,\,\tau-1,\,(t-1):\tau,\, \&\mathrm{c}.\big)\\
&+\&\mathrm{c}.
\end{aligned}\right\}\ (9)$$

each part being affected with the factor $(-1)^{N-1}$ derived from the differentiations performed upon

$$x_{\rho-1} \cdot x_{\rho}^{r-1} \cdot x_{\sigma}^{s} \cdot x_{\tau}^{t} \div x_1^{N}$$

$$x_{\rho}^{r} \cdot x_{\sigma-1} \cdot x_{\sigma}^{s-1} \cdot x_{\tau}^{t} \div x_1^{N}$$

$$x_{\rho}^{r} \cdot x_{\sigma}^{s} \cdot x_{\tau-1} \cdot x_{\tau}^{t-1} : x_1^{N}$$

&c.

2nd. Suppose ρ, the lowest index, is 2, then the term

$$x_{\rho-1} \cdot x_{\rho}^{r} \cdot x_{\sigma}^{s} \cdot x_{\tau}^{t}$$

must be rejected, because $x_{\rho-1}$ becomes x_1, which is excluded from appearing in any numerator. But then, *per contra*, in this case there will be a portion of the coefficient derivable from the differentiation of the denominator of the term

$$(-)^{N-2} \cdot \frac{\big((r-1):2, \ s:\sigma, \ t:\tau\big) x_2^{r-1} \cdot x_{\sigma}^{s} \cdot x_t^{\tau}}{x_1^{N-1}},$$

where

$$(N-1) = 1 + (r-1)2 + s \cdot \sigma + t\tau + \&c.$$

This portion will be

$$(-)^{N-1} \cdot (N-1) \times C\big((r-1):2, \ s:\sigma, \ t:\tau\big),$$

or, which is the same thing,

$$C\big(1, \ (r-1):2, \ s:\sigma, \ t:\tau, \&c.\big),$$

and therefore the portion of the coefficient corresponding to $x_{\rho-1} \cdot x_{\rho}^{r} \cdot x_{\sigma}^{s} \cdot x_{\tau}^{t}$, &c. is supplied from another source, and the expression $\mathfrak{S}$ remains good for *all* values of ρ, σ, τ, &c., and consequently, by virtue of the second lemma, is equal to $C(r:\rho, s:\sigma, \ t:\tau, \&c.)$; and thus we see that if the law assumed is true for $\dfrac{d^{r} \cdot u}{dx^{r}}$, it remains true for $\dfrac{d^{r+1} \cdot u}{dx^{r+1}}$, as was to be shown. And as it is evidently true for $r = 1$, it is true generally.

Lincoln's Inn Fields,
October 13, 1854.

Postscript. The formula expressing Burman's law may be exhibited as follows: x_r will still be understood to denote $\dfrac{d^r x}{du^r}$, and $C\{p, q, \ldots m\}$ will, as before, denote the number of distinct modes of combining $p + q + \ldots + m$ things in sets of $p, q, \ldots m$ at a time; so that, *ex. gr.* $C\{2, 2, 4, 4, 4\}$ will denote

$$\frac{1 \times 2 \times 3 \ldots \times 16}{(1 \cdot 2)^2 \cdot (1 \cdot 2 \cdot 3 \cdot 4)^3} \cdot \frac{1}{1 \cdot 2} \cdot \frac{1}{1 \cdot 2 \cdot 3}.$$

Let now $n-1$ be broken up *without restriction in every possible way* into parts, and let $r, s, t \ldots l$ denote one such system of parts so that $r+s+t+ \ldots +l=n-1$, r, s, &c. being all *actual* positive integers. Then is

$$\frac{d^n u}{dx^n} = \Sigma C\{\overline{1+r}, \overline{1+s}, \overline{1+t}, \ldots \overline{1+l}\}$$

$$\times \frac{1}{x_1^n} \cdot \left\{ \frac{-x_{1+r}}{x_1} \cdot \frac{-x_{1+s}}{x_1} \cdot \frac{-x_{1+t}}{x_1} \ldots \frac{-x_{1+l}}{x_1} \right\},$$

than which nothing more clear and simple can be desired or imagined. And so more generally, if we make, as before, $r+s+t+ \ldots +l=n-g$, and give g in succession every different value from 1 to n, we shall have

$$\frac{d^n \mathfrak{S}}{dx^n} = \Sigma\Sigma\left(\{(\overline{1+r}, \overline{1+s}, \ldots \overline{1+l}), \overline{g-1}\} \frac{d^g \mathfrak{S}}{du^g} \right.$$

$$\left. \times \frac{1}{x_1^n}\left(\frac{-x_{1+r}}{x_1} \cdot \frac{-x_{1+s}}{x_1} \ldots \ldots \frac{-x_{1+l}}{x_1} \right) \right),$$

where $\left((\overline{1+r}, \overline{1+s}, \ldots \overline{1+l}), \overline{g-1}\right)$ means the number of ways in which $\overline{1+r}, +\overline{1+s}+ \ldots +\overline{1+l}+\overline{g-1}$ elements can be partitioned off into groups of one kind containing respectively $(1+r), (1+s), \ldots (1+l)$ of the elements, and into a group of another kind containing the remainder $(g-1)$ of the elements. This distinction of the groups into two kinds has no effect upon the result except when $g-1$ is equal to any of the numbers $(1+r), (1+s), \ldots (1+l)$. If we write, according to the notation above employed, $\overline{1+r}, \overline{1+s}, \ldots \overline{1+l}$ under the form $a{:}\alpha, b{:}\beta, \ldots c{:}\gamma, \{(\overline{1+r}, \overline{1+s}, \ldots \overline{1+l}), \overline{g-1}\}$ will represent

$$\frac{a\alpha + b\beta + \ldots + c\gamma + g - 1}{G(a)(G\alpha)^a \times G(b)(G\beta)^b \ldots G(c)(G\gamma)^c . G(g-1)}.$$

This more general theorem may of course be demonstrated by a similar method to that employed in the text for the case of $\mathfrak{S}=u$, for which all the terms in the expansion vanish except those in which $g=1$.

I have, since this paper was sent to the press, obtained a new solution of the far more difficult and interesting question of the change from one *system* of independent variables to another system. I say a new solution, because one has already been *virtually* effected, but under a form leaving much to be desired, by the great Jacobi in his Memoir *De Resolutione Æquationum per series infinitas*, Crelle, vol. vi. 1830. In my solution, a remarkable species of quantities, to which I give the name of Arborescent Functions, make their appearance in analysis for the first time.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 6, 1854.—Thomas Graham, Esq., V.P., in the Chair.

THE following paper was read :—" On a peculiar Arrangement of the Sanguiferous System in *Terebratula* and certain other Brachiopoda." By W. B. Carpenter, M.D., F.R.S.

In a memoir " On the Minute Structure of Shell," read before the Royal Society January 17, 1843, (and subsequently embodied in a " Report " on the same subject, prepared at the request of the British Association for the Advancement of Science, and published in its Transactions for 1844,) I first announced the fact, that the ' punctations ' which had been previously noticed on the exterior of many Brachiopodous shells, both recent and fossil, are really the orifices of *tubular perforations*, which pass directly through each valve, from one of its surfaces to the other (fig. 1).

Having subsequently obtained specimens of *Terebratulæ* in which the soft parts of the animals had been preserved, in connexion with their shells, I ascertained that these passages are occupied in the living state by membranous cæca, *closed externally*, but opening on the *internal* surface of the shell, and filled with minute cells of a brownish hue. Recollecting that Professor Owen, in his account of dissections of some species of *Terebratula* and *Orbicula* (Transactions of the Zoological Society, vol. i.), had spoken of an unusual adhesion of the mantle to the shell in these Bivalves, it occurred to me that this adhesion might be due to a continuity between the mantle and these cæcal tubuli; and I carefully sought for evidence of such a structure. In this, however, I was entirely unsuccessful; for the mantle, when stripped from the shell, presented no appearance whatever of having transmitted any such prolongations into its substance; on the contrary, it was evidently continued over the mouths of the cæca with which it was in apposition; and I frequently found its external surface (*that* in contact with the shell) covered *in patches* with cells exactly resembling in size and aspect those contained within the cæca. I was equally unsuccessful in the attempt to trace any other connexion between these cæca and the soft parts of the animal; so that, although their importance in its œconomy scarcely admitted of doubt, the nature of their function remained entirely unknown. The idea that they had any connexion with the formation of the *shell* itself, seemed to be completely negatived by the fact, that in a large proportion of the group of BRACHIOPODA, no such perforations exist; notwithstanding that their shells, in every other feature of minute structure, are exactly accordant with that of *Terebratula.*—The foregoing results were communicated to the British Scientific Association in 1847, and were embodied in the Second Part of my " Report " published in its Transactions for that year.

The physiological importance of the characters of ' perforation '

or ' non-perforation ' has become continually more obvious, as the principles on which the subdivision of the group of Brachiopoda should be founded, have been gradually settled by those who have concerned themselves with its systematic arrangement; and in particular, the *universal presence* of the perforations in the shells of the family *Terebratulidæ,* contrasted with their equally universal absence in those of the family *Rhynchonellidæ,* unequivocally marked its relation to the general conformation of the *animals* of these subdivisions.

Having been requested by Mr. Davidson to undertake a more detailed investigation than I had yet made, into the minute structure of the shells of Brachiopoda, for the sake of throwing still further light upon the classification of the group, I applied myself afresh to the solution of the problem, and believing that I have succeeded in ascertaining the import of this curious feature in the organization of Terebratula and its allies, I beg to offer an account of my results to the Royal Society.

The membrane which is commonly spoken of as ' the mantle,' and which may be stripped from the shell by the use of sufficient force to overcome its adhesions, must, I maintain, be considered as really its *inner layer* only; for I find that an outer layer exists, so intimately incorporated with the shell as not to be separable from it without the removal of its calcareous component by maceration in dilute acid. When thus detached, this outer layer is found to be continuous with the membrane lining the perforations in the shell (fig. 1 *b*); so that their tubular cæca are, in fact, prolongations of the *real* external surface of the mantle. The adhesion of the *inner* to the *outer*

Fig. 1.
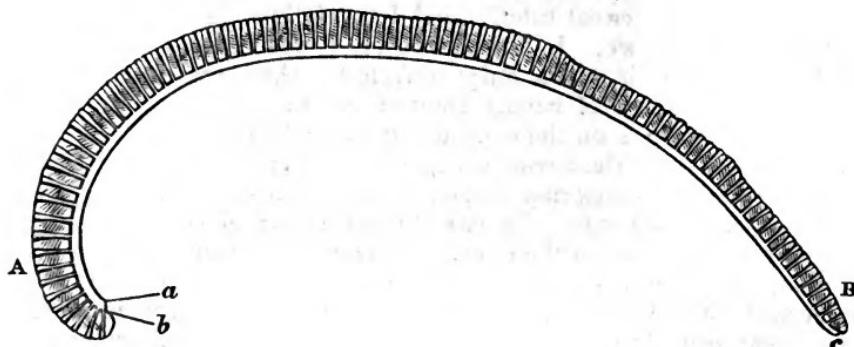
Diagram of the intra-palleal sinus-system of *Terebratula,* with its cæcal prolongations into the shell;—A, B, section of valve; *a,* inner layer of mantle, *b,* outer layer in contact with the shell, and giving off cæca; *c,* continuity of the two at margin of valve.

layer (which Professor Owen, not being aware of the existence of an outer layer, interpreted as an adhesion of the *mantle* to the *shell*) does not extend to the whole of the contiguous surfaces, but is limited to certain bands or spots,—the two layers of membrane, in the intervals between these, being separated by a set of irregular spaces, freely communicating with one another, and with the cavities

of the cæca, so as to form a rude network. This arrangement is peculiarly well marked in *Terebratula caput-serpentis*, as shown in the figure (fig. 2); and to those who are familiar with the condition of the circulating apparatus in the inferior Mollusca, it is scarcely possible not to recognize in it a ' sinus-system,' corresponding to that which is formed in the Tunicata by the partial adhesion of the second and third tunics to each other.

Considered under this point of view, the cæcal structure (as was first suggested to me by my friend Mr. T. H. Huxley) bears a close resemblance to the vascular prolongations, which, in many Ascidians, pass from the sinus-system into the substance of the ' test ;' the chief difference lying in this,—that whilst each of the vascular prolongations into the ' test ' of the Ascidians contains both an *afferent* and an *efferent* canal,—no such distinction ordinarily manifests itself in these prolongations of the intra-palleal sinus-system of *Terebratula*, although I have met with indications of it in *Crania*. Their cæcal character, however, is by no means opposed to the views I am now giving of their physiological nature; for it has been shown by M. de Quatrefages, that the prolongations of the ' general cavity of the body,' which pass into the branchiæ and other appendages of Annelida, transmitting to them its nutritive fluid for aëration, are always cæcal, notwithstanding that they are sometimes distributed as minutely as blood-vessels*.

Fig. 2.
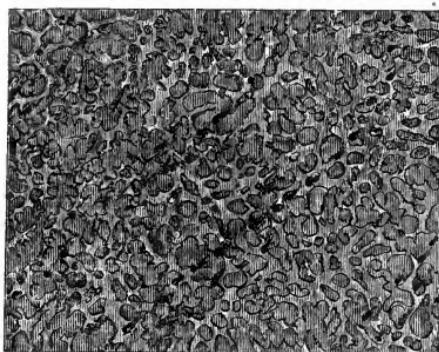
Sinus-system of *Terebratula caput-serpentis* (as shown by the grinding away of the shell, without detaching the mantle), being a network of canals formed by the adhesion of the two layers of the mantle at certain spots, leaving passages around them.

On this interpretation, the cells which are found within the cæca, and in the spaces between the contiguous surfaces of the two layers of the mantle, are to be regarded as *blood-corpuscles*, and they correspond in size and appearance (so far as can be determined by specimens preserved in spirits) with the blood-corpuscles of Ascidian and Lamellibranchiate Mollusks.

The sinus-system from which this collection of cæca proceeds,

* Ann. des Sci. Nat., 3ᵉ sér., Zool., tom. xviii. p. 307.

appears to be altogether distinct from the vascular apparatus of the (so-called) ' mantle,' (that is, according to my interpretation, *of the inner layer* of the mantle) which has been described by Professor Owen; but it probably communicates with the ' common sinus ' at the back part of the visceral chamber, which is stated by Professor Owen to receive the blood, not only from the palleal sinuses of the dorsal and ventral valves, but also from " other sinuses that there fill, line, and seem to form, the visceral or peritoneal cavity*."

It cannot be deemed improbable, then, that the apparatus in question is *branchial* in its nature ; and that it is designed to provide for certain tribes a more special means of aërating the blood, than is afforded by that distribution of blood to the general surface of the mantle, which is common to the entire group.   This view of its respiratory office is confirmed by an observation communicated to me by Professor Quekett; viz. that the discoidal opercula which cover the external orifices of the cæca, and which, though adherent to the periostracum, are not structurally continuous with it, present appearances in young shells, which seem indicative of the existence of a fringe of cilia round each, designed to produce currents of water over the extremities of the cæca.

The resemblance which these cæcal prolongations of the sinus-system into the shell of the *Terebratula* bear to the vascular prolongations of the sinus-system into the test of certain *Ascidians*, is not without its parallel in another group, which (as pointed out by Mr. Hancock, Ann. of Nat. Hist. vol. v. p. 198) is intimately related to that of Brachiopoda,—namely, the *Bryozoa*.   The stony walls of the ' cells ' which invest the soft bodies of many species of *Eschara, Lepralia*, &c., are marked, like the shells of Terebratulæ, with punctations, which are really the orifices of short passages extending into them from their internal cavity, as sections of these structures demonstrate.   These passages I have found to be occupied by prolongations of the visceral sac, which is the only representative of a circulating system among these animals; and they thus convey the nutrient fluid which this contains, into the substance of the framework formed by the calcified tunics of these animals.

I need not here enlarge upon the additional value which these structural and physiological considerations afford, to the character of " perforation " or " non-perforation " in the shells of Brachiopoda. The importance of this character in systematic arrangement will plainly appear, I think, from the details which I have published in the Introduction to Mr. Davidson's Monograph already referred to.

June 15, 1854.—The Earl of Rosse, President, in the Chair.

The following papers were read :—

" An Inquiry into some of the circumstances and principles which regulate the production of Pictures on the Retina of the Human Eye, with their measure of endurance, their Colours and Changes."— Part II.   By the Rev. William Scoresby, D.D., F.R.S.

This second part of the author's inquiries concerning phænomena

* See Mr. Davidson's Monograph on the " British Fossil Brachiopoda," published by the Palæontographical Society, vol. i. p. 15.

in optical spectra, embraced the results in respect of *colour* in the images impressed on the retina, as derived simply from the influence of *light*.

The optical spectra from white, gray, or black opake objects under faint illumination, or of ordinary windows or apertures transmitting low degrees of light, were usually found to be without colour. But ordinary daylight, and, much more, the light from bright sunshine (as is well known), yield *chromatic spectra* of vivid or brilliant hues. By viewing with slightly closed eyes, the pictures impressed on the retina by a few seconds' steady gazing at some fixed point of an illuminated object, and noting the various effects, disappearances and changes, a considerable number of characteristic phænomena were elicited, and the effects of a variety of modifying circumstances satisfactorily determined. The most prevailing influences in modifying the phænomena—whatever other causes might tend to the production of variation in the colours—were found to be referable to differences in the degree of intensity of the external light, in the extent of time occupied in gazing at the illuminated object, in the quantity of light penetrating the chamber of the eye whilst examining the spectra, and in the normal condition of the eye itself. These, with other modifying circumstances, had been somewhat elaborately investigated.

Different degrees of light, whether reflected from white objects, or transmitted by colourless glass, had obviously the tendency to yield differences in the colours of the primarily developed pictures on the retina, with corresponding varieties in the nature and number of the subsequent changes. Thus the viewing for a few seconds of an aperture in a window the size of a pane of glass, whilst all the rest was covered with a thick brown-paper screen, gave, *with a low degree of daylight*, transparent pictures of a *dingy* orange, olive, yellow-gray or bluish black tint, changing, most usually, into a rusty-tinted blackish spectrum, and disappearing, for the most part, in a minute of time or less. From *medium degrees of daylight*, the primary pictures embraced a considerable variety of colours, such as crimson-pink, purple-pink, violet, purple, indigo, blue,—the blue being the highest in the scale of intensity. The most marked changes, commencing with *blue*, were usually from blue to red, or to crimson, olive, black fading into blackish gray. In certain cases rapid and evanescent glances were had of several intermediate colours. The general photochromatic effects of the *higher degrees of light*, such as from a clear sky in full sunshine, were far more uniform than those from inferior light. The spectrum first elicited, even after viewing a window or window-aperture for three or four seconds only, was almost always *green*, with the character of illuminated transparency ; the shades of colour however varied with the intensity of the impression. The picture always appeared within four or five seconds after closing the eyes, and when the light had been strong and the gazing continued for a quarter of a minute or more, the picture would burst out almost instantly. The restoration of the picture in new colours, after the vanishing, had very much the

character and appearance of the dissolving views effected by the magic lanthorn. *The frame* of the window or aperture, and the cross-bars, were always pictured in colours different from those of the panes, besides a fine marginal line of another colour dividing the glass and the frames. These *consequential colours,* constitute, as is well known, a remarkable feature in the phænomena. They have generally a certain complementary relation, or tendency to such, to the colours of the primary picture. Thus in the clear green or blue spectrum of a window, derived from strong illumination, the remainder of the field of the eye will generally, in *the first* instance, be covered with a ground of glowing crimson, with cross-bars similar, and purple edgings; and when the picture changes to crimson or red, the antagonistic tint will also change, perhaps to purple, or orange or brown. The original spectra were found to fade away at intervals, often of tolerable equality, such as of eight or nine seconds, disappearing perhaps for two or three seconds, and then reappearing under, generally, some change of shade or tint, through an extent of very numerous repetitions. The changes of colour from the bright or emerald green, as very frequently traced, went rapidly through yellow-green, yellow, orange, red, scarlet, crimson and brown, or olive. And this series, it is observable, is particularly accordant, in respect to the principal or fundamental colours, with that of the prismatic spectrum from green to yellow, orange and red. These visual photographs, besides having the sharpest definition, and often the most brilliant illuminated colours, were found to possess, under strong intensities of impression, a remarkable degree of permanency—extending sometimes to endurance for an hour or longer after the act of gazing.

Investigations on the relation of the photochromatic developments to the time of gazing, gave results in many respects corresponding with those derived from differences in the degree of external light. Thus the higher colours of the spectral series elicited by strong light, could, within certain limits, be also developed by more continuous gazing with inferior light : so that the pink-coloured spectrum derived from ten to twenty seconds' gazing in low degrees of light, could be elicited by a single glance under bright sunshine. The results, therefore, were clearly in relation to the intensity of the impression ; and, taken in the form of a general proposition, we shall not be far wrong, perhaps, in considering the intensity of impression as the product of the time of gazing into the relative quantity of light admitted by the aperture.

The relation of the colours primarily elicited to the intensity of the impression, yielded (comparatively and roughly taken) the following series,—crimson-pink, purple-pink, purple, blue, green, the latter being the produce of the highest intensity tried.

As in the foregoing researches, the relative degrees of light were but broadly assumed, whilst the comparative experiments comprised a variety of differences affecting the photochromatic results, another series of experiments on the simple effects of degrees of light was instituted, in which all these other differences were elimi-

nated. In this series the quantity of light was varied by partial or sectional screens of glass, or other transparent or semitransparent substances. The results were particularly satisfactory,—different tints or shades of colour being obtained by the same view and in the same spectrum of a window-aperture, when different thicknesses of window glass were placed in the several sections (six in number) into which the aperture was divided.

A beautiful example of the chromatic effects of partial and varied screening of light on the optical spectrum elicited, was incidentally obtained by viewing an aperture in the clouds, when the sky was otherwise densely covered. After gazing for a few seconds on the middle of this aperture, the spectrum, as viewed with gently closed eyes, exhibited a singular variety of the richest tints according to the differences in the light screened off by the edges of the cloud and by certain little patches within the aperture. The spectrum resembled the variegation and richness of colouring as elicited in certain transparent or semitransparent substances when examined by polarized light.

The experiments on binocular and multiple spectra, as described in Part I. of the author's paper, being repeated under degrees of light adequate for yielding *colour*, gave pictures, in many cases, of much interest and beauty. The multiple spectra, however, which proved the most strikingly beautiful, were derived from the sun, which was viewed *indirectly*, and on occasions, near setting, in winter, when the intensity of its light was duly subdued by passing through a dense condition of atmosphere. Under such circumstances, images, sometimes in 100 to 150 repetitions, were impressed on the retina by rapid glances at the sky immediately around the sun. These were taken by quick movements of the head, winking intermediately, at the rate of 60 to 120 impressions in the minute ; and the result, when viewed with closed eyes, presented a splendid spectacle like a cluster of coloured stars, or rather of round planetary discs, brilliant in green, yellow, orange, red, crimson and purple !

Besides the experiments thus far described, in which the spectral images were viewed, for the most part, with gently closed eyes kept steadily in the direction in which the objects were gazed on,—the differences, which were often very remarkable, produced by alterations in the quantity of light admitted into the chamber of the eye whilst the image was viewed, were also investigated. Sometimes the smallest change in the light thus transmitted was found to alter greatly the character of the spectrum. In certain cases, the compressing of the eyelids, or the mere passing of the hands betwixt the eyes and the light, would serve to change a negative picture into a positive, or the colours, as viewed in the usual way, into their complementary tints.

The paper concludes with a considerable series of deductions, applications and general results.—1. As to the *elucidation* yielded by these ocular spectra, of the theory of vision.—2. Of the principles of binocular and simple vision.—3. Of the action of the retina for the obliterating of impressed images, and the recovery of a normal con-

dition.—4. Of the nature of certain disturbing and dazzling effects of vision by strong light.—5. Of the phænomena of certain spectral illusions.—6. As to the *practical use* of the process of examining the ocular spectra, for the determination of quantities of light relatively intercepted by different portions or thicknesses of glass or other transparent media.—7. For assisting in the determination of the relative degrees of illumination of lamps, candles, &c., and of quantities of light reflected from opake objects.—8. For aiding in the selection and harmonizing of colours in ornamental and decorative departments of art.—9. For the examination of the condition of the interior of the eyes in certain states of disease. The author having had the opportunity of trying this process in case of amaurosis, found that it afforded a perfect picture of defects in the surface of the retina of the eyes separately, when there was no visible defect, and when the patient had no other perception of a diseased eye, or patch on the retinal surface, except the partial distortion or interruption of vision. Founded on this, the author suggests a plan of *scotometrical* examination of retinal defects, by which not only the accurate form and relative proportions of diseased patches on the retina may be determined, but their actual dimensions may probably be deduced.

"Note on Nitro-glycerine." By A. W. Williamson, Ph.D., F.C.S., Professor of Practical Chemistry in University College.

This compound is formed by acting upon glycerine with a mixture, in equal volumes, of concentrated nitric and sulphuric acids, the glycerine being added by a few drops at a time.

It is heavier than water, in which it is slightly soluble, and is soluble in alcohol and in æther.

From its proneness to decomposition in drying, even by the air-pump, a complete analysis could not be made, but a qualitative examination of the relative amounts of carbon and nitrogen gave the following results :—

	1.	2.	3.	4.
Volumes of mixed gases	101	91·5	99	97
Volumes of nitrogen not absorbed by potash	32	30·5	34	33
Carbonic acid absorbed by potash	69	61	65	64

	1.	2.	3.	4.	5.
Mixed gases	178	194	173	194	192
Nitrogen	61	66	58	65	65
CO^2	117	128	115	129	127

From these results the following formula was deduced :—

$$C^6 H^8 O^6 + 3NO^5 = C^6 \begin{matrix} H^5 \\ 3NO^4 \end{matrix} O^6 + 3HO.$$
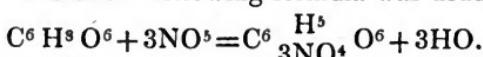
It would therefore appear that 3H are replaced by $3NO_4$.

On boiling this compound with concentrated solution of potash, it is decomposed into glycerine and nitrate of potash.

"On a new Phosphite of Æthyle." By A. W. Williamson, Ph.D. &c.

The following results were obtained by Mr. Railton in an investigation undertaken in connexion with the idea that the water

of constitution discovered by Wurtz may be conceived as basic. The processes for preparing the compound are thus described by Mr. Railton.

1st. When three atoms of absolute alcohol are acted upon by one atom of PCl³, this compound is formed. The alcohol is introduced into a retort which is connected with an apparatus for upward distillation, and the retort is surrounded with a freezing mixture. The terchloride is then added drop by drop, the whole is then gently heated for some time, the vapour being allowed to run back into the retort. It is now distilled and the portion which comes off between 140° C. and 196° C. collected and redistilled, that portion being preserved which boils between 188° and 191° C. The quantity of pure æther obtained by this process was not large, and there was left in the retort a considerable amount of PO³ and other products, which on further heating evolved inflammable phosphuretted hydrogen.

2nd. This æther is obtained with the greatest facility from æthylate of soda and terchloride of phosphorus.

I introduce into a thirty ounce stoppered retort about a pint of æther, which must be perfectly free from alcohol and from water. The æthylate of soda is then added, and as much PCl³ is taken as is necessary to form chloride of sodium and phosphite of æthyle. The æther is absolutely necessary, for without it, the action of the PCl³ is so violent, as to set fire to the æthylate.

The PCl³ is introduced into the mixture of æther and æthylate of soda through a long funnel, which is drawn to an extremely fine point, by which means it enters drop by drop into the mixture, thus avoiding the violent action which otherwise occurs.

The retort should be kept quite cool and frequently shaken. If these precautions are neglected considerable loss is experienced.

When the whole of the PCl³ has been added, the æther is distilled off by a water-bath. The retort is then transferred to an oil-bath which is gradually heated up to about 240° C. The whole of the distillate obtained by the oil-bath is collected in a dry receiver, and as it is prone to decomposition if distilled in air, it is distilled in an atmosphere of hydrogen, the portion which comes off at 188° C. is the phosphite of æthyle. I may here notice the remarkable fact, that this substance has two boiling-points, as doubtless have many other bodies, if distilled under similar circumstances. In air it boils at 191° C., while, as I said before, it boils in hydrogen at 188° C. Its specific gravity is 1·075.

3rd. The reaction which occurs on the formation of this æther may be represented by the following formula:—

$$3NaO,\ C^4H^5O + PCl^3 = 3NaCl + 3C^4H^5O,\ PO^3.$$
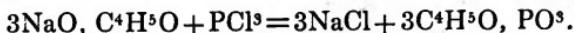
The carbon and hydrogen were estimated in the usual manner by oxide of copper, the phosphorus as follows. A weighed portion of the æther was introduced into a twelve ounce stoppered bottle; concentrated nitric acid was poured upon it, and the bottle allowed to stand in a warm place, loosely stopped, for several days. When nitrous fumes no longer appeared, the oxidation of the phosphorous

acid was deemed to be complete. The acid liquid was then saturated with ammonia, some chloride of ammonium and sulphate of magnesia then added, and the mixture well shaken. It was allowed to stand for some time, when a precipitate of phosphate of magnesia and ammonia was formed; this was washed, dried, and ignited, and the amount of phosphorus calculated from the result. These are the results.

Grms.		CO_2.		HO.	$2MgO, PO_5$.
·2405 æther gave		·3784 and		·1920	,,
·5115	,,	·8047 and		·4155	,,
·4513	,,	,,		,,	·302
·4110	,,	,,		,,	·278

From these results the following per-centages are calculated.

	Required.	Found.	
C^{12}	43·11	42·91	42·89
H^{15}	8·98	8·87	9·03
P	19·16	18·92	19·10
O^6	28·75	29·30	28·98
	100·00	100·00	100·00

These results being satisfactory as regards the formula, the density of the vapour was then ascertained and found to be in strict accordance with theory. The method of taking the vapour densities of bodies liable to oxidation was described by me about twelve months ago, in the Chemical Society's Quarterly Journal. It was used in the following experiments.

1st. Weight of globe filled with air at 53° F. and 30·2 in. barometer, 188·213 grs.

Weight of empty globe, 186·313 grs.

Weight of globe and vapour at 521° F. and 30·3 in. barometer, 192·387 grs.

Capacity of globe at 60° F., 6·00 cub. in.

Residual hydrogen ·05 cub. in.

Capacity of globe at 521° F., 6·40 cub. in.

Six cubic inches of air at 53° F. and 30·2 in. barometer, become at 60° F. and 30 in. barometer 6·12 cub. in., and weigh 1·90 gr.

·05 cub. in. hydrogen at 60° F. become ·094 cub. in. at 521° F., and weigh ·002 gr.

$6·40 - ·094 = 6·306$ cub. in. vapour at 521° F., which at 60° F. and 30 in. barometer $= 3·376$ cub. in.

Hence $192·387 - ·002 = 192·385 - 186·313 = 6·072$ grains, the weight of 3·376 cub. in. vapour.

$$100 \text{ cubic inches.} . = 179·86 \text{ grs.}$$
$$100 \text{ cubic inches air} = 31·01 \text{ grs.}$$

The density is therefore 5·800, from which it appears that its combining measure is four volumes.

Density by calculation $= 5·763$.

A second experiment gave 5·877.

This substance has a highly offensive odour, it burns with a bluish

white flame, is soluble in water, alcohol, and æther, and is slowly decomposed in contact with air.

On boiling phosphite of æthyle with concentrated solution of baryta, in water, it is decomposed into alcohol and a salt which varies according to the amount of baryta used. If one atom of the æther be treated with one of baryta, a crystallized salt is produced on evaporation, the carbon and hydrogen in which are, according to an analysis I have just completed,—

	Found.	Required.
Carbon	20·354	24·158
Hydrogen ..	5·356	5·050
Baryta	36·880	37·090

In that marked 'required' I have supposed the salt to bear the following formula and to be completely anhydrous, $2C^4H^5O$, BaO, PO^3, but if we suppose that four atoms of water are present in the salt analysed, the relation will stand thus:

Found.	Required.
20·354	20·453
5·356	5·540

The formula would then be $2C^4H^5O$, BaO, $PO^3 + 4HO$.

When two atoms of baryta are made to act upon one atom of the æther, a salt is obtained which does not crystallize, and it may be evaporated in air without sensible decomposition. This salt is perfectly neutral to test paper; when dry it is a white friable deliquescent mass, the formula of which will be C^4H^5O, $2BaO$, PO^3. If an excess of baryta is used, a white salt is thrown down on boiling, which I suppose to be HO, $2BaO$, PO^3.

I have prepared another compound with three equivalents of amyle. This was obtained from amylate of soda by an analogous process to that described for the phosphite of æthyle.

Analysis has pointed out the formula $3C^{10}H^{11}O$, PO^3. Like phosphite of æthyle it is easily decomposed on being heated in air; heated in hydrogen it is more stable and then boils at 236° C. It is soluble in æther and in alcohol, but only slightly soluble in water.

"On the Constitution of Coal-tar Creosote." By Prof. Williamson.

For some years past it has been a debated question among chemists, whether the peculiar body originally described by Reichenbach as creosote, and subsequently analysed by Ettling and others, has any real existence, or whether the properties which were attributed to it are not to be more correctly ascribed to the hydrate of phenyle, which can be obtained in a state of great purity from at least one sort of commercial creosote by mere distillation, and which possesses in an eminent degree the antiseptic properties for which creosote is remarkable.

With a view of obtaining some light on this question, Mr. Fairlie undertook, in the laboratory of University College, an investigation of the portions of coal-tar creosote which boil higher than the hydrate of phenyle. The result of his experiments has been to show that a

body homologous to hydrate of phenyle may be obtained from the crude creosote, in fact the next term of the series above hydrate of phenyle itself. Some qualities of commercial creosote contain a greater quantity of this *hydrate of cresyle* (as it may be termed) than others; and it is most advantageously prepared from those portions which in the first distillation come over between 200° Cent. and 220°. After a great number of fractional distillations, a colourless, highly dispersive liquid is obtained, boiling at 203° Cent., and possessing the composition represented by the formula $C^{14}H^8O^2$.

This hydrate of cresyle resembles the corresponding phenyle compound in most of its properties; but it may be easily distinguished from that compound by its almost complete insolubility in aqueous ammonia.

When gradually mixed with sulphuric acid, it becomes of a beautiful rose-colour, and gives rise to sulpho-cresylic acid.

The action of nitric acid upon hydrate of cresyle is very violent, and almost explosive if the acid is used in a concentrated state and at so high a temperature as the common atmospheric; even very dilute nitric acid transforms the compound into a brown tarry mass from which no definite substance can be extracted. By cooling some nitric acid in a frigorific mixture and allowing some similarly cooled hydrate to fall into it drop by drop and with constant agitation, a red-coloured solution was obtained, which by dilution with water and subsequent neutralization by potash yielded a crop of short needle-shaped crystals of an orange-red colour, and possessing a greater solubility in water than the salt of carbazotic acid. This salt was found by analysis to possess the composition of a homologue of carbazotate of potash; so that it is the potash salt of a hydrate of cresyle in which three atoms of hydrogen are replaced by hyponitric acid,

$$C^{14} \begin{matrix} H^4 \\ (NO^4)^3 \end{matrix} O, KO.$$
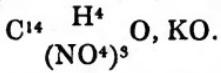
The same acid was obtained by the action of nitric acid upon an alcoholic solution of the hydrate containing urea; but in attempting to repeat this experiment on a larger scale the mixture became hot, and the whole of the substance was destroyed with almost explosive violence.

When treated with pentachloride of phosphorus this hydrate of cresyle is decomposed in like manner with the hydrate of phenyle, as described by Mr. Scrugham, yielding a chloride of cresyle and a phosphate of the same radical.

By the action of this phosphate in an alcoholic solution of acetate of potash, a peculiar oleaginous body is obtained possessing an odour entirely different from that of the hydrate, and decomposable by potash with production of acetate and cresylate.

A similar reaction ensues when the phosphate is distilled with æthylate of potash, and a cresylate of æthyle is thus obtained.

In the numerous distillations which were performed for the purification of the hydrate of cresyle, some circumstances were observed which led to a suspicion that the body undergoes a change of

composition, either through the distillation itself, or by some influences accompanying it. These circumstances were,—1st. A tarry residue, from a liquid which when introduced into the retort was perfectly colourless. 2nd. The formation of a small quantity of water in the commencement of such a distillation, though none was contained in the substance used. 3rd. The gradual lowering of the boiling-point of the whole liquid by a great number of distillations. These facts, taken in conjunction, naturally suggested that the oxygen of the air contained in the retort might act upon the substance, and thus gradually reduce it to hydrate of phenyle.

In order to test the correctness of this hypothesis, the atmospheric air was expelled from the distilling apparatus by dry hydrogen gas, and the distillation performed in a pure atmosphere of this gas. A great number of distillations performed in this manner were at exactly the same temperature, and all the other anomalies were simultaneously removed. It was however found that the liquid always boiled at a lower temperature in hydrogen than in atmospheric air, the difference being about 2° Cent., and this without any alteration of the pressure on the surface of the boiling liquid. A similar fact was noticed in the distillation of hydrate of phenyle, and also of some other liquids.

LXVI. *Intelligence and Miscellaneous Articles.*

ON A NEW METHOD OF ALKALIMETRY.
BY ASTLEY PASTON PRICE, PH.D., F.C.S.

HAVING had occasion, some time since, to test the comparative value of the ordinary alkalimetrical processes, I was somewhat surprised at the discrepancies in the results I obtained, and also at the somewhat difficult and tedious manipulation necessary, when comparatively accurate determinations were required. The most serious impediment in carrying out the usual alkalimetrical processes, arises, as is well known, from the liberation of carbonic acid, the presence of which, even in exceedingly small quantity, being sufficient to mask the point of saturation, and to prevent the indication of the presence of either an excess of acid or of alkali.

After having experienced the difficulties attendant on its presence, and the almost impossibility of rapidly and entirely expelling the liberated carbonic acid from solution, it appeared to me most desirable to seek an alkalimetrical process, in which the carbonic acid should be expelled previous to determining the saturating power of the alkali under examination. It further appeared to me, that in the alkalimetrical processes now in use, there existed another very serious inconvenience, in that the per-centage of alkali was determined directly, and not indirectly; that is, that the alkali was estimated, and not the impurities which might be therein contained. This may perhaps be better understood by an example. Pure carbonate of soda contains about 58·5 per cent. of alkali, but com-

mercial carbonate contains only about about 50 per cent. Now in the several alkalimetrical processes employed in the arts, the 50 per cent. of alkali is estimated, and not the $8 \cdot 5$ per cent. of impurities. Constituting, as these impurities invariably do, by far the smaller proportion of the commercial alkalies or alkaline carbonates, it will in practice, I believe, be found advantageous to employ a method which, by indicating the amount of impurities present, will give the available proportion of alkali.

As I have previously remarked, the primary difficulty to be overcome is the entire expulsion of carbonic acid, the presence of but a small quantity of which entirely prevents accurate determination, not only owing to the change of tint produced by its presence on a solution of litmus, but owing to the diminished sensibility of litmus thus tinted.

In the hope of avoiding those sources of error to which I have alluded, and of facilitating the determination of the true per-centage of alkali, I adopted a method of alkalimetry which in substance is the following :—To the alkali under examination is added a known excess of a normal solution of oxalic acid ; and after the expulsion of carbonic acid from the solution by boiling, the excess of oxalic acid remaining is determined by means of a standard solution of ammonia.

Some objection might be made to the employment of a solution of ammonia ; but it will be found that a dilute solution of ammonia, if kept in properly constructed apparatus, will remain more constant than might be expected. The standard solutions of ammonia and of oxalic acid are most easily prepared by means of a standard solution of sulphuric acid, care being taken that perfectly pure acid be employed, and that the amount of real acid be carefully determined.

Having prepared the standard solutions of a desired strength, the determination of an alkali or of an alkaline carbonate may be thus effected :—10 grs. of an alkaline carbonate, carbonate of soda for example, after having been placed in a flask, a solution of oxalic acid corresponding to 10 grs. of pure carbonate of soda is added ; the solution is then boiled until the expulsion of carbonic acid be effected, when the solution is diluted with distilled water ; and after the addition of a few drops of a solution of litmus, the excess of oxalic acid is determined by a standard solution of ammonia. The excess of oxalic acid remaining will of course indicate the impurities present, or the absence of alkali, which, by deduction from the quantity originally taken, will give the amount of available alkali.

Care must be taken that the solution be only tinted with litmus, and not too deeply coloured, as the more feeble the coloration within certain limits, the more easily detected is the change of tint produced by an excess of alkali or of acid.

I have found it necessary to employ distilled water for diluting the solutions, failing, as I have done, to obtain accurate results with other water, owing to the presence of carbonic acid. The preparation of standard solutions is so well understood, that it is unnecessary to enter into further details.

The apparatus I have found most convenient for containing and preserving standard solutions, and more particularly solutions of ammonia, consists of a vessel similar to a wash-bottle, to which is attached an India rubber bulb; so that when it is desired to fill the burette, it is only necessary to compress the bulb. This arrangement affords great facility for replenishing the burette; and by placing a piece of India rubber tubing, closed at one end, on the jet, an air-tight reservoir for the solution is obtained.—*Chem. Gaz. for* Nov. 15, 1854.

ON CUMINIC ALCOHOL. BY M. KRAUT.

The author has treated oil of cummin with a solution of potash in alcohol, and resolved it in this manner into cuminic acid and its alcohol.—Liebig's *Annalen,* xc. p. 384.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1854.

Chiswick.—October 1. Foggy: drizzly: overcast and fine: foggy. 2. Dense fog: very fine. 3, 4. Very fine. 5. Fine, but windy. 6. Rain. 7. Overcast. 8. Cloudy and fine. 9. Fine: overcast. 10. Foggy: exceedingly fine: rain at night. 11. Fine throughout. 12. Clear. 13. Foggy: very fine. 14. Foggy: hazy: slight drizzle. 15. Foggy and drizzly: cloudy: rain. 16. Very fine. 17. Foggy: rain. 18. Rain: overcast and windy. 19. Fine: rain at night. 20. Showery. 21. Cloudy. 22. Densely clouded: fine. 23. Fine: clear. 24. Very fine: heavy rain. 25. Constant rain. 26. Clear and very fine. 27. Frosty and foggy: very fine. 28, 29. Very fine. 30. Foggy: very fine: cloudless. 31. Exceedingly fine.

Mean temperature of the month .... 48°·20
Mean temperature of Oct. 1853 .... 49 ·99
Mean temperature of Oct. for the last twenty-eight years ... 50 ·06
Average amount of rain in Oct. .... 2·68 inches.

Boston.—Oct. 1, 2. Foggy. 3. Cloudy. 4. Fine. 5. Fine: rain P.M. 6. Cloudy. 7, 8. Fine. 9. Cloudy: rain A.M. 10. Fine. 11. Cloudy: rain A.M. 12, 13. Fine. 14. Cloudy. 15. Cloudy: rain P.M. 16. Fine. 17. Cloudy: rain P.M. 18. Rain A.M. and P.M. 19. Cloudy. 20. Cloudy: rain A.M. and P.M. 21. Fine. 22. Cloudy: rain A.M. 23, 24. Fine. 25. Cloudy: rain A.M. and P.M. 26, 27. Fine. 28. Cloudy. 29. Fine. 30. Cloudy. 31. Cloudy: rain P.M.

Sandwick Manse, Orkney.—Oct. 1. Cloudy A.M.: clear P.M. 2. Rain A.M. and P.M. 3. Showers A.M.: clear P.M. 4. Cloudy A.M. and P.M. 5. Showers A.M.: clear P.M. 6. Hail-showers A.M.: clear P.M. 7. Showers A.M.: clear P.M. 8. Clear, fine A.M.: fog P.M. 9. Rain A.M.: cloudy P.M. 10. Clear, fine A.M.: showers P.M. 11. Showers A.M.: sleet, showers P.M. 12. Clear, fine A.M.: clear P.M. 13. Drizzle A.M.: clear P.M. 14. Clear, fine A.M.: cloudy P.M. 15—17. Showers A.M. and P.M. 18. Hail-showers A.M.: cloudy P.M. 19—22. Showers A.M. and P.M. 23. Bright A.M.: showers P.M. 24. Clear, frost A.M.: clear, frost, aurora P.M. 25. Cloudy, frost A.M.: sleet-showers, aurora P.M. 26. Sleet-showers A.M. and P.M. 27. Bright A.M.: cloudy P.M. 28. Rain A.M.: cloudy P.M. 29. Cloudy A.M.: clear P.M. 30, 31. Clear A.M. and P.M.

Mean temperature of Oct. for twenty-seven previous years ... 47°·68
Mean temperature of this month .... 46 ·39
Mean temperature of Oct. 1853 .... 48 ·66
Average quantity of rain in Oct. for fourteen previous years . 5·07 inches.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month. 1854. Oct.	Barometer Chiswick Max.	Barometer Chiswick Min.	Barometer Boston 8½ a.m.	Barometer Orkney, Sandwick 9¼ a.m.	Barometer Orkney, Sandwick 8½ p.m.	Thermometer Chiswick Max.	Thermometer Chiswick Min.	Thermometer Boston 8½ a.m.	Thermometer Orkney, Sandwick 9¼ a.m.	Thermometer Orkney, Sandwick 8½ p.m.	Wind Chiswick 1 p.m.	Wind Boston	Wind Orkney, Sandwick	Rain Chiswick	Rain Boston	Rain Orkney, Sandwick
1.	30·222	30·130	29·73	29·82	29·86	67	41	50	55	54	sw.	calm	w.	·01	...	...
2.	30·073	29·851	29·66	29·77	29·55	74	36	51	56	53	sw.	calm	se.	...	...	·37
3.	29·798	29·736	29·27	29·65	29·73	65	31	57·5	43	46	w.	wnw.	nw.	...	...	1·08
4.	29·856	29·694	29·44	29·54	29·03	62	52	49	46	52	sw.	calm	w.	...	...	·29
5.	29·585	29·510	29·06	29·03	29·23	71	50	58	44	42½	sw.	sw.	nw.	·01	·03	...
6. ○	29·571	29·491	29·15	29·48	29·91	58	47	51	42½	40	ne.	calm.	nnw.	·52	...	...
7.	30·080	29·869	29·56	30·09	30·06	59	42	52·5	42	42	e.	ene.	sw.	...	...	·30
8.	29·960	29·847	29·68	29·97	29·83	60	52	50	47	44	se.	se.	sw.	...	...	...
9.	29·823	29·662	29·25	29·64	29·60	70	43	57	53½	52	sw.	se.	ne.	·03	·05	·14
10.	30·014	29·992	29·52	29·62	29·61	65	54	57	55½	48	sw.	w.	nw.	...	...	...
11.	30·297	30·009	29·55	29·81	30·18	62	28	55	49	49	nw.	nw.	w.	·13	·24	·27
12.	30·482	30·454	30·06	30·32	30·18	58	29	41	50½	49½	n.	calm	s.	...	...	...
13.	30·419	30·338	30·02	30·08	30·06	60	34	38	51½	49	n.	sw.	sw.	...	...	...
14. ☽	30·291	30·211	29·83	30·00	29·75	56	44	53	52½	57	w.	sw.	ssw.	·06	·08	·13
15.	30·115	30·058	29·70	29·82	29·84	54	37	55	47	46	sw.	nw.	w.	·16	...	·18
16.	30·028	29·671	29·65	29·79	29·68	56	28	40	43	38	ne.	calm	calm	·04	...	·17
17.	29·396	29·258	29·10	29·79	29·93	49	41	41	48	41½	e.	n.	n.	·03	·20	·05
18.	29·419	29·227	28·92	29·90	29·80	50	31	48	40½	37½	n.	nw.	n.	·61	·10	·08
19.	29·786	29·469	29·35	29·44	29·07	51	40	41	47	48	w.	w.	nw.	·04	·26	·15
20.	29·433	29·356	28·97	29·06	29·42	57	39	47	44½	47	w.	nnw.	nnw.	...	·04	·23
21. ●	29·638	29·439	29·20	29·48	29·24	57	45	46	45	48	nw.	nnw.	wnw.	...	·02	·34
22.	29·550	29·303	28·84	29·11	29·13	60	31	54·5	45½	43	w.	wsw.	w.	...	...	·30
23.	29·400	29·356	28·96	29·04	29·06	60	28	40	43½	39½	w.	w.	sw.	...	...	·05
24.	29·444	29·093	29·03	29·06	29·02	56	33	37	37	34½	sw.	nnw.	calm	·52	·23	...
25.	29·219	28·974	28·77	29·02	29·15	50	29	37	39½	39½	ne.	w.	nnw.	·44	...	·28
26.	29·793	29·523	29·15	29·37	29·61	54	24	37	43½	42	w.	w.	nw.	...	...	·27
27.	30·253	29·980	29·80	29·93	29·94	53	31	33	42½	45	s.	s.	sse.	...	...	...
28. ☾	30·287	30·175	29·83	29·61	29·73	58	26	38	50	51½	sw.	s.	ssw.	...	...	·50
29.	30·163	30·098	29·65	29·66	29·81	56	34	42	51	47½	sw.	sse.	ssw.	...	...	·02
30.	30·172	30·061	29·74	29·95	29·72	65	36	52	48	51	sw.	s.	sse.	...	...	·01
31.	30·159	30·035	29·81	29·70	29·83	67	33	50	50	45	sw.		s.	·01	...	...
Mean.	29·895	29·744	29·43	29·630	29·628	59·35	37·06	47·3	46·90	45·88				2·61	1·25	5·21

INDEX to VOL. VIII.

END OF THE EIGHTH VOLUME.

1

D   D

C   C

⅓ rd actual size.

B   B

d

3 2 1 0

E   4 3 2 1 0

A

E

d

2

B

A

⅕ th actual
size.

3

B

90
80
70
60
50
40
30
20
10

C

N   D

M

P   O

A

F   K   E

L

H

G

Fig. 1.

Fig. A.

Fig. 2.

Fig. B.

Fig. 3.

Scale for *Figs.* 1 & 2.

0   1   2   3   4   5   6   7   8   9   10 *decimetres.*

Scale for *Figs.* A. B. & 3.

0   1   2   3   4   5 *decimetres.*

A   B   C

D   E   F